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### DESCRIPTION OF THE PLATES

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### DESCRIPTION OF THE TEXT FIGURES

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### THE GOLD MASK OF KING KOFI OF ASHANTI

*This magnificent head is reproduced, by permission of the Trustees of the Wallace Collection, to mark the golden jubilee of MAN. It formed part of the treasure of King Kofi Kalkalli, who was defeated in the First Ashanti War in 1874 by Sir Garnet Wolseley's expedition. With other fine gold and gold-plated objects, it was bought by Sir Richard Wallace in that year and was in the great art collection left by his widow to the nation. Though never before published, it is without doubt the finest of all extant Ashanti antiquities. It is a hollow cast, about  $\frac{1}{4}$ -inch thick, and is here reproduced at actual size; it weighs 3 lb. 6 oz. The area of the left temple is somewhat battered. This reproduction will be seen to the best advantage when a strong light is reflected from it.*



# THE ORIGIN OF MAN

by

PROFESSOR SIR JOHN L. MYRES, O.B.E., D.LITT., D.SC., F.B.A., F.S.A.

*Founder and first Honorary Editor of MAN*

I At the request of the Honorary Editor, I have set down the following record of the origin of MAN, on the occasion of its jubilee issue. I regret that some early letters are no longer to be found, and I desire to thank Miss Stallman for her researches in the Institute's minute books.

Formerly the weekly *Athenæum* was the sole means of publication for short papers and letters on literary and historical subjects. As the scope and public interest of archaeological studies increased, the *Academy*, also weekly, was established in 1869, and under the editorship of John Sutherland Cotton was supported by many of the scholars most concerned. But it failed to compete on these lines with the *Athenæum*, and changed hands in 1896.

Soon after this, Professor Flinders Petrie wrote to me at Oxford suggesting that I should establish a journal to succeed the *Academy* under the title *Man*, and perform for the humanities the services rendered by *Nature* for the natural sciences. I secured the goodwill of about fifty linguists, historians, and archaeologists, and raised a guarantee fund, some of the principal contributors to which desired to remain anonymous.

In correspondence with an eminent firm of publishers, I was assured (i) that my health would not permit me to conduct such a journal (ii) that my financial resources (undisclosed) were inadequate, (iii) that I had no time to spare for it. Only after some plain speaking did I elicit an estimate, for a weekly journal in the same format as *Nature*. It was about twice the estimate for the same work from a publisher in Leyden, who even promised the services of an Englishman as proof-reader. Even this estimate however was higher than the guarantors would accept. I remained however in touch with my well-wishers.

In December, 1899, the Anthropological Institute, founded under that name in 1871, entered on a fresh period of activity, under the presidency of Charles Hercules Read, of the British Museum. At the same time, Ormonde Maddock Dalton, also of the Museum, was succeeded as Honorary Secretary by William Crooke, then living near Cheltenham. It had always been a chief duty of the Honorary Secretary to edit the *Anthropological Journal*, and some former editors had collected 'Miscellanea' to fill blank pages in the last sheet. These notes, which were not submitted to Council like the formal 'communications', included reviews of books presented to the library; they had recently increased in volume, and appeared at the end of each quarterly part of the *Journal*.

In 1898 the Council increased the size of the *Journal* from demy octavo to royal octavo (Council Minutes, 8 March, 1898) and the parts were issued half-yearly. Some Fellows regretted the longer interval between parts.

In January, 1900, when I succeeded Crooke as Honorary Secretary, I found in the office many contributions, large

and small, including reviews and books awaiting review. In May, Council (i) increased the number of pages allowed to the *Journal*, (ii) agreed to collect the 'Miscellanea' at the end of the volume, and increase their amount, (iii) allowed Fellows who were willing to pay for offprints of 'Miscellanea' to receive each 16-page sheet of them in advance, in addition to what would be included in their copy of the *Journal* (23 October, 1900).

These preliminaries made it possible to submit to the Council (on the same date) a proposal for the establishment of *Man* as a monthly record of anthropological science, published under the direction of the Anthropological Institute. There was no mention of an editor, who was in fact the Honorary Secretary as before. Each number consisted of 16 pages of miscellanea, with one full-page plate, and a cover for Table of Contents, Institute notices and advertisements. Each monthly part was to be sold to Fellows at 6d. and to the public at 1s.; each Fellow still receiving a complete file of MAN bound up with his *Journal* at the end of the year. This was held to be necessary if MAN was to rank as a publication of the Institute, but it did not encourage Fellows to become subscribers to MAN.

The finance of this separate issue was however kept distinct from that of the Institute, to meet the scruples of the Honorary Treasurer. Subscriptions to MAN, payments from guarantors, and the Institute's payments for the sheets received for inclusion in the *Journal* were paid into a separate account at the Westminster Bank in Hanover Square, close to the Institute's office at that time; the Honorary Secretary drew cheques on this account to meet outgoings; and the printing was entrusted to Messrs. Eyre and Spottiswoode, Ltd., not to the Institute's printers, Messrs. Harrison & Sons, Ltd. At the end of the year the Honorary Secretary submitted a statement of receipts and expenses to Council.

The first number of MAN appeared on 1 January, 1901, so that the number of the volume was that of the calendar year. There were printed 1500 copies; 246 subscriptions were received, there were 27 exchanges, 221 copies were sold, and 859 distributed by way of advertisement. Valuable help was rendered in distribution by the Assistant Secretary, N. W. Thomas, who had recently succeeded an old and faithful servant of the Institute, J. A. Webster.

In 1902 the financial position permitted the occasional addition of supplementary pages, in double columns, for reviews and correspondence; and the relations between MAN and the *Journal* were revised in detail (18 April, 1902). So far from depleting the reserves of the *Journal*, as some had feared, its volumes for the early years of MAN were larger than ever before. The membership of the Institute was also rising rapidly.

Though the MAN accounts for 1902 had been duly



accepted by Council (18 December, 1902), the Honorary Treasurer 'being in doubt about certain items' refused to pay to the Honorary Secretary's account sums due by the Institute for copies of MAN supplied for inclusion in the *Journal*. This seemed to me intolerable, and I resigned the Honorary Secretaryship, and therewith all responsibility for MAN. When Council met (27 January, 1903), it ordered the full payment without further enquiry, but after some discussion accepted my resignation. My successor as Honorary Secretary was T. A. Joyce, of the British Museum, who remained Editor of MAN till 1923, though he ceased to be Secretary in 1918. From 1915 to 1926 H. S. Harrison edited the *Journal*, but not MAN.

On 8 November, 1904, the Institute decided to discontinue

the supply of MAN to all Fellows as part of the Institute's proceedings; and later decided that though the Institute accepted responsibility for publication (14 February and 4 March, 1905), MAN was not to be considered in future to be a part of the *Journal* (10 January, 1905). The Guarantee Fund was closed on 28 February, 1905. Later still the postage, which was at first charged to the Institute's general account, like that of the *Journal*, was debited to MAN, which had begun to show a small annual balance.

From 1923 E. N. Fallaize was Honorary Secretary and Editor till 1931, when I resumed the editorship in an emergency, while President, and held it till I was succeeded in 1947 by W. B. Fagg, the Honorary Secretary. The *Journal* had its own editor from 1915.

## THE ORIGIN OF PICTORIAL ART

by

G. D. HORNBLOWER, F.S.A.

2 On the assumption that drawings on the cave walls of the palaeolithic age were magical instruments for the promotion of success in hunting, the question quickly arises of how man arrived at the idea that solid bodies could be intelligibly represented by mere outlines. The answer seems to lie in the clay models of animals discovered in the caves of Montespan (see *Revue Anthropologique*, Vol. XXXIII, 1923, pp. 533-45 and 545-50; also Sollas, *Ancient Hunters*, 3rd ed., pp. xxvii-xxxvi; a handy account is given by Dorothy Davison in *Men of the Dawn*, ch. XIV). These models, of the Gravettian age, figured animals of the same nature as those delineated in the caves of Altamira and elsewhere and must have been made with the same magical intention as the drawings. The solution of the problem as suggested below might seem to be founded on an extreme stretch of imagination, but it is to be noted that all the early stages sketched here were actually found in the caves and support the theory outlined in this article.

In the age with which we are concerned a strong feeling of the supernatural was impressed on all mankind, pervading thought and imagination alike; closely interwoven with it was the idea of magic, notably that of 'similar.' When, therefore, a man of specially strong imagination chanced to be handling a lump of the pliant clay with which he was familiar, he was seized with amazement as he saw it take, under his fingers, a shape like that of an animal. It would emerge, for him, as a definite instance of the magic of similars, having some kind of mysterious living connexion with the actual animal, thus placing it under his power—a welcome boon, indeed, for a community of hunters. The hardly formed image would be retouched till it came nearer the shape of the living creature, much as the ancient Egyptians, finding a piece of tabular flint bearing some resemblance to an animal important to them as an image of a god or divine mascot, would make the resemblance closer by skilfully chipping

round the edges of the object (see Capart, *Primitive Art in Egypt*, pp. 152-5 and figs. 115-8; and on p. 185, with fig. 146, the account of a flint pebble thus adapted).

The great step from modelling to simple outline may be explained by the evidence from Montespan as follows. The clay figures would quickly collapse and lose all value for the hunters; to prevent this mishap they were made to lean against a cave wall, but to remain supported they would have to be pressed against the wall on their inner side, which would thus be lost to the eye. However, the outer half would retain for an observer the same effect as the whole and would be credited with the same magical virtue; it was also easier to fashion and would be readily adopted in the place of the whole, thus giving rise to a method of high relief which is well exemplified in the cave of Tuc d'Audoubert (Davison, p. 134). This method would next be developed into one of low relief by some flattening of the figures with the view of economizing both material and labour. But the reliefs were very fragile; the clay would dry up and crumble away but would often leave traces of the outline of the figures on the walls on which they had been modelled. Such outlines would, in the thought of the operators, still retain the powers of the original clay models and the practice of engraving them on the walls with flint tools, to preserve them, came into use. Then men, having learnt to obtain colour from mineral products of the earth, adapted them to the figures on the walls, superseding the far harder process of engraving with flint, and pictorial art was born.

The discovery at Montespan is perhaps unique, but the future may possibly reveal other sites like it; they must, however, be exceedingly rare from their nature and circumstances, and also because the length of time between introduction of the method of modelling and the far more practicable one of outlining would probably be comparatively short (as indeed may be inferred from the finding of many stages together at Montespan).



The use of clay must have been common in the palæolithic age, when properly kneaded and dried, if only to provide food-containers and perhaps even, temporarily, for small quantities of water. Some of the crude pottery found in a prehistoric settlement in the Fayoum by Miss Caton-Thompson, though of neolithic age, may give us an idea of what the shapes of the palæolithic may have been, especially the rectangular specimens.

An effort at still further advance in the art of the time was recently reported by Professor Dorothy Garrod in the *Illustrated London News* of 16 July, 1949, describing a specimen of human portraiture found by her and Mlle Suzanne de Saint Mathurin in a cave at Angles-sur-l'Anglin together with many limestone plaques worked in the same polychrome technique. The sculptured portion is treated in the pecking technique shown so well in Bushman art; it was known in Ancient Egypt, at least as long ago as twenty-three centuries B.C., when it appears on the so-called Colossi of Min (see MAN, 1946, 103, with note 4).

In the matter of technique the engraved limestone pebble found at La Colombière, and described and illustrated by Professor W. Koppers in MAN, 1950, 132, seems, as he suggested, to be a kind of sketch book for the sacred, or half-sacred, man charged with the magical cave decoration, and thus throws an interesting gleam of light on the whole matter. Perhaps some of the decorated limestone fragments reported by Professor Garrod as found in the same region as the human portrait mentioned above may have served in like manner as sketches. Similar preliminary fragments have frequently been found in Ancient Egypt; many are shown in the Cairo Museum and elsewhere.

W. H. Riddell, an experienced hunter of big game as well as an artist, has pointed in his pamphlet *Altamira* (Oliver and Boyd, 1938) to another possible technique, suggesting that the artist often used dead animals as models. He insists that the ancient draughtsman was always deeply

immersed in intense feeling for his work, endowing it with 'passionate vitality' (pp. 17-26). To this expression of lively sympathy is added a striking exposition of the mode of vision of primitive hunters as he has himself observed them.

The origin then seems clear. It does not lie, as has often been suggested, in any innate instinct in man: no one, infant or older, can lay a hand on any article serviceable for drawing—brush, pencil, chalk or anything else—and use it with any purpose of representation unless he has previously, at no matter how young an age, seen the process conducted or explained. Here the psychologist may have a say, and may in any case find profit in examining any elements of human mentality that may be dormant in the method of development in art thus described.

Finally, this development seems to have left material traces in modern times. In Greek, for example, the word used for writing, *γράφειν*, meant originally 'to graze or scratch,' while Hebrew has two words, one meaning originally to incise (*sōfar*) and the other (*kethaba*) 'to write,' as with a brush. Now the Israelites when settled in their country had a great culture on each flank, the Assyrian and the Egyptian, and from each they absorbed elements into their own culture (see Professor Hooke in Vol. VI (published 1947) of *The Clarendon Bible*, pp. 3, 4 and 7). Thus they adopted both the word derived from the Assyrian method of writing, namely incising in clay, and the Semitic word indicating the Egyptian method of writing with a brush. The result has been well studied and analysed, but the words were used indifferently throughout the Old Testament, and no basis of differentiation, though suspected, has been established.

The reed and the pen or split-quill point follow on the brush and thus we have the process plain, as above surmised, from clay to pen (for details of the earlier process, see the excellent Schweich Lectures on *Semitic Writing* given by G. R. Driver in 1944).

## PRIMITIVE TRANSPORT VEHICLES IN ULSTER\*

by

J. M. MOGEY AND G. B. THOMPSON

3 Ireland, partly by reason of its location at the western limits of Europe and partly because of the survival there of active communities of small farmers, plays an important role in folklore studies. The richness of its heritage of oral literature is vouched for by the magnificent collections of the Irish Folklore Commission in Dublin, but the field of material culture remains curiously neglected.<sup>1</sup> This short note on material gathered in Ulster is presented less as evidence of the need for more work in this field than as an indication of the vitality of traditional skills and to show that, despite a phenomenal increase in mechanized agriculture with consequent social repercussions,<sup>2</sup> the earlier traditions still persist. Under the rapidly changing conditions of the modern world this persistence cannot any longer be taken for granted.

\* With four text figures

The illustrations are all of transport devices and fall into four main categories. Examples of the simplest of all transport vehicles, the single-runner sled or guide runner, appear in fig. 1. Fig. 1a was used to transport the wheelless plough from field to field. These swing ploughs, first recorded in 1802,<sup>3</sup> have now almost disappeared and such tiny slides as this are difficult to discover. Examples made from a tree branch have been noted in Co. Tyrone; the illustration is of iron, from Glenloughan, Co. Down. The antiquity of this device far antedates A.D. 1800; Swedish examples have been placed in the Bronze Age<sup>4</sup> and the widespread geographical distribution is also a sign of great antiquity. Berg gives many instances from Scandinavia and quotes sporadic occurrences elsewhere.<sup>5</sup> He concludes that 'the single-runner sled . . . may be assigned to a prehistoric culture element within the



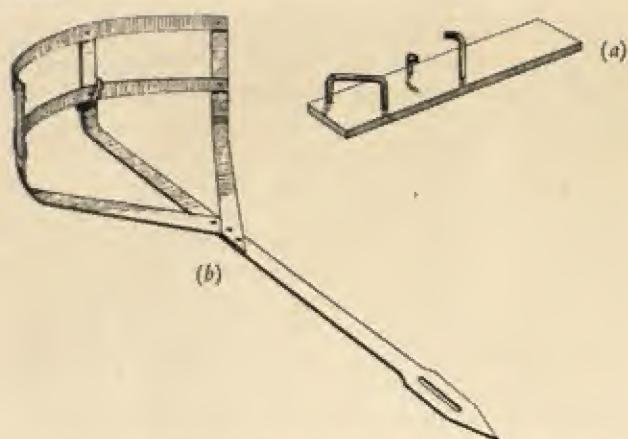


FIG. 1. (a) PLOUGH SLIPE, GLENLOUGHAN, CO. DOWN,  
(b) HAY SLIPE, GLENARIFF, CO. ANTRIM  
*Drawings by G. B. Thompson*

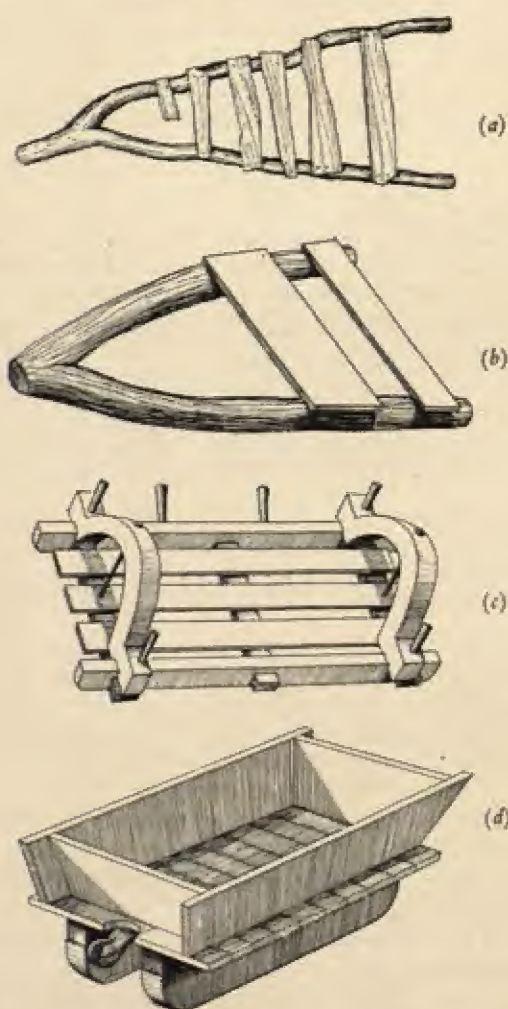


FIG. 2. (a) FORKED-BRANCH SLIPE, GLENDUN, CO. ANTRIM  
(LENGTH 5 FEET), (b) BOULDER SLIPE, BALLYMAGEOGH, CO.  
DOWN (AFTER ESTYN EVANS), (c) TURF SLIPE WITH TIPPING  
HANDLES, DRUMCROW, CO. ANTRIM, (d) SOIL OR MANURE  
SLIPE, MOURNE MOUNTAINS, CO. DOWN

circumpolar regions.<sup>6</sup> Its appearance (or re-appearance) in a new guise in Northern Ireland during the nineteenth century is in our opinion evidence of the vitality of the folk culture there. The elaboration of the single runner shown in fig. 1*b* from Glenariff, Co. Antrim, is further indication of the use of old devices for new needs. An 'invention' of a local blacksmith, it is intended to pull the small field haystacks to the farm haggard. When the

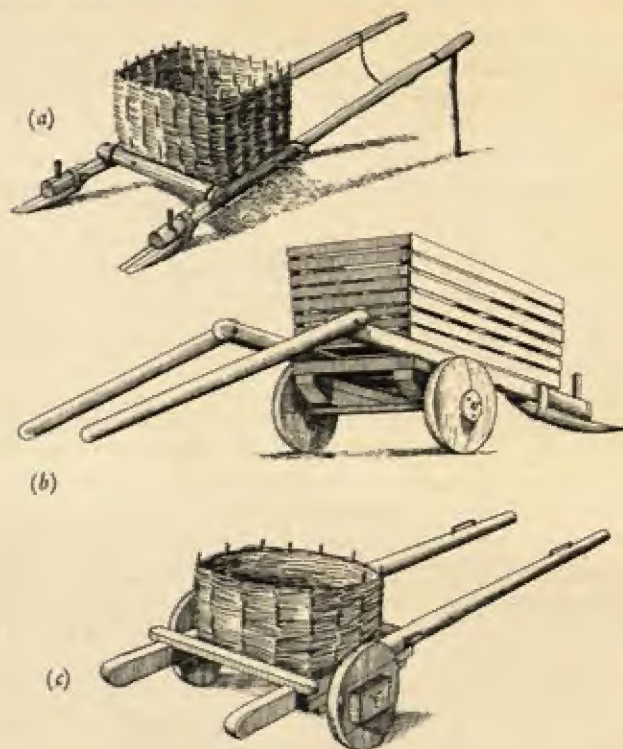


FIG. 3. (a) SLIDE CAR WITH CREEL, GLENAAN, CO. ANTRIM,  
(b) SLIDE CAR WITH WHEELS ATTACHED, WATERFOOT, CO.  
ANTRIM, (c) SLIDE CAR WITH WHEELS, GLENELLY RIVER VALLEY,  
CO. TYRONE

inward journey is complete the horse is hitched to the ring at the back and the contraption pulled from under the load.

The double-runner sledge ('slipe' is the local word) occurs more commonly. Leaving aside the wider question of the place of such vehicles in the evolutionary pattern, which has already been sufficiently discussed,<sup>7</sup> figs. 2*a* and *b* may be regarded as prototype models. They are intended for moving stones from the arable plots and other heavy loads. The carpenter-built models which follow show the need for devices to tip the load off these low-slung vehicles once they become specialized. Two methods of achieving this end are illustrated: fig. 2*c*, a turf (peat) slide with handles from Drumcrow, Co. Antrim, and *d*, a soil or manure sledge with high, close-set runners from the Mourne Mountains, Co. Down.

Wheelless carts and their subsequent modifications form the third group. The early type, fig. 3*a*, was drawn in Glanaan, Co. Antrim, in 1944; these are now rare. An



'improved' model, fig. 3b, comes from the same district; it is adapted to the transport of peat from the high-level bogs down to the settled area in the valleys. The wheels help going uphill and on the return journey the skids act as a brake. The composite nature of the specimen in fig. 3c, from Glenelly, Co. Tyrone, is clear enough and is made more pointed by the fact that the three-piece wheels are bought in the next county.

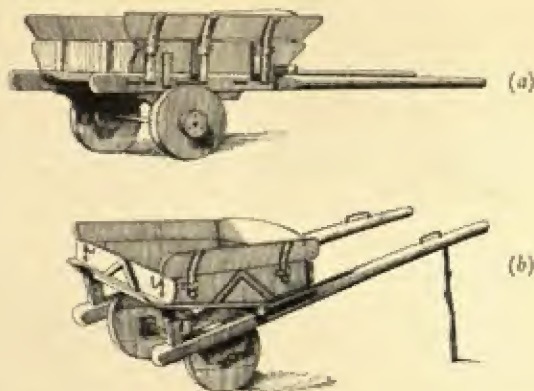


FIG. 4. (a) BLOCK-WHEELED CART, GLENELLY RIVER VALLEY, CO. TYRONE, (b) WHEEL CAR, CO. ANTRIM, ESPECIALLY AT HEAD OF GLENARM VALLEY, ABOUT DRUMCROW AND MULLAGHSANDAL

Finally we illustrate the two principal types of wheeled cart that are old-established in Ireland, an 'inside' car (fig. 4a), also from Glenelly, Co. Tyrone, and an outside car

or wheel car from Glenarm, Co. Antrim. These small-capacity carts with their tiny wheels were the most advanced mode of goods carriage known in Ireland until the beginning of the nineteenth century. The two-wheeled Scottish cart, common in the domestic linen area of the Lagan valley by 1812,<sup>8</sup> spread throughout the rest of the province during this century and today these wheel cars, drawing 10 to 12 cwt. as against the 22 cwt. of the Scottish cart,<sup>9</sup> survive only in regions of steep slopes.

A study of these humble implements in their many manifestations suggests that these small farm communities are drawing on a heritage with its roots deep in the past and actively using it to solve problems that arise in modern everyday life. This is a process too little studied and its contributions to the evolution of machine technology in this country may not be without importance.

#### Notes

<sup>1</sup> The more important studies include: A. C. Haddon, *The Study of Man*, (1908); L. Mulhausen, *Cork Hist. and Archaeol. Soc.*, Vol. XXXVIII (1933); Åke Campbell, *Béaliodeas* (1935), and *Folkiv*, Vol. I (1937), pp. 207-34, and Vol. II (1938), pp. 173-96; E. Estyn Evans, *Irish Heritage*, (1942).

<sup>2</sup> MAN, 1948, 98.

<sup>3</sup> Rev. J. Doubourdieu, *Stat. Survey of the Co. of Down* (1802), p. 50.

<sup>4</sup> Gösta Berg, *Sledges and Wheeled Vehicles* (1935), p. 16.

<sup>5</sup> Berg, *op. cit.*, p. 28.

<sup>6</sup> Berg, *op. cit.*, p. 34.

<sup>7</sup> Berg, *op. cit.*; Cyril Fox, *Antiq.*, Vol. V (1931), pp. 185-199; R. U. Sayce, *Primitive Arts and Crafts* (1933); A. G. Haudricourt, *Rev. de géog. humaine et d'éthnol.*, Vol. I (1948), pp. 54-64.

<sup>8</sup> E. R. R. Green, *The Lagan Valley* (1949), p. 50.

<sup>9</sup> I. J. Herring, *Ulster J. Archaeol.*, Ser. 3, Vol. VII (1944), pp. 42-6.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

### MAN, 1901-1951

4 Fellows of the Institute, reading Sir John Myres's characteristic account of the rather amœba-like genesis of MAN from the 'Miscellanea' section of the *Journal* under his guiding hand, may well rejoice that half a century later they can offer him their congratulations and thanks, and perhaps their apologies on behalf of their predecessors for the too hesitant faith placed in those days in Mr. John Myres of New College.

If the *Journal*, together with the monographic literature, be regarded as providing the bricks of the anthropological building in Great Britain—and, with the contributions of other countries, in the world at large—then MAN may fairly claim comparison with the mortar that cements the edifice so that it may stand as a single whole despite the individuality of its parts. Its many short articles and reviews, highly specialized though many of them are, help to complete that continuum which is the Study of Man. The avoidance of any rigid planning and of arbitrary narrowing of MAN's field of interest has been pursued as a matter of policy, in the belief that this will contribute to the illumination of anthropological studies by new facts and new theories, far beyond the specialisms in which they first appear. A novel application in the study of human morphology of D'Arcy Thompson's principles of growth and form (MAN, 1949, 119) may suggest to some a valid and valuable approach to the study of primitive art. And if material culture and archaeology may sometimes be useful

ancillaries to social anthropology, it is also true that social anthropology may be a useful ancillary to them. None of these is the sun round which the others as planets revolve.<sup>1</sup>

Reference to the first article in 'Anthropological Reviews and Miscellanea' for 1900—in effect the prospectus of MAN, which was to appear in the following January—shows that the plan proposed for it was so well and generously conceived that the remarkable advances of the past half-century in many branches of our science have not called for any fundamental changes in it. Indeed, the fifty years' perspective is enough to remind us that these advances, startling though some of them may have seemed to their originators and to others, were none of them revolutionary; all were solidly founded on the achievements of the past, and all have been, or are in process of being, assimilated in the main stream of the progress of anthropology.

A matter which without doubt bulked large in the mind of MAN's founder is the fitness of a monthly publication as a vehicle of scientific controversy (of which, in the best sense, he has long provided an unattainable ideal). It is perhaps to this more than to anything else that MAN owes its special place in world anthropological literature, and through a series of happy coincidences the correspondence columns of this jubilee issue exceptionally well illustrate this function. May controversy long flourish freely in MAN, and may its pages never have to carry that compulsory 'controversy' which is a mark of the bondage of science!



## Note

<sup>1</sup> In symbolical recognition of the necessary coherence and interdependence of the parts of anthropology and of MAN's function of serving the whole study of man, there appears for the first time on the cover of this issue a small device to replace the lower of the two swelled rules which have bounded the summary Contents. This device is a drawing of a well-known type of brass gold-dust weight, from Ashanti in the Gold Coast; it represents two crocodiles sharing a single belly, and illustrates one of the best-known Ashanti proverbs, which runs (in Rattray's rather uninspired translation): 'Bellies mixed up, crocodiles mixed up, we have between us only one belly, but if we get anything to eat it passes down our respective



FIG. 1. ASHANTI PROVERB GOLDWEIGHT

gullets.' 'This proverb,' Rattray explains, 'is often quoted to illustrate any falling away from the due observance of the family system, when one member of it becomes greedy and wishes to seize everything for himself.' In its present application, it may be interpreted as meaning that many different specialized approaches are possible and desirable in the anthropological field, provided that their underlying unity is not lost sight of.

The drawing is based on a fine specimen illustrated here (at actual size) from the collection of Mr. Webster Plass.

## A Note on Typography

**5** The type face used in the printing of MAN is that known as Bembo, a revival by the Monotype Corporation (in 1930) of the roman face cut for Aldus Manutius and first used by him in Venice in 1495 for the tract *De Aetna ad Angelum Chabrielem Liber*, written by Cardinal Pietro Bembo. The original face was the prototype on which the French sixteenth-century punch-cutters (Garamond, Tory, Estienne) based their designs, and the modern recutting has been acclaimed by typographers from its first appearance up to the present time. The Bembo italic stems from the Roman Chancery scripts of the Papal scribes, and was modelled on an italic cut by Giovanni Antonio Tagliente, who had himself been a Papal scribe. The disciplined italic is particularly easy and pleasing to read, and has the special virtue, for an anthropological publication, of turning the large incidence of native words into an asset rather than a liability to the printed page.

Apart from its aesthetic merits, the Bembo face has certain more utilitarian advantages, notably from the point of view of readability. This is no doubt due in part to the 'old face' non-vertical shading of its letters and to the fact that Bembo is 'small on the body,' so that it has the appearance of leaded type. Again, the reduced capitals (shorter than the 'ascenders' of the lower-case) would make even a page of Carlyle's over-capitalized prose readable without discomfort, and, besides, allow more room for the addition of certain diacritical marks over capitals.

Original Articles are set in 11-point Bembo, Proceedings of the Institute, Shorter Notes and Obituaries in 10-point, and Reviews

and Correspondence (as well as footnotes) in 9-point. The Bembo bold face, used for the titles of all except Original Articles, does not include an italic, so an alternative fount is used where necessary. The marginal numbers, a distinctive feature of MAN from the beginning, are the figures of Eric Gill's Perpetua Tiding, which accords very well with Bembo. The front cover has been reset, mainly in Bembo, for this issue.

The Hon. Editor cannot let this golden jubilee issue pass without recording his own and the Institute's appreciation of the great services of MAN's printers (since 1941), William Clowes and Sons, Ltd., of Beccles. No editor could wish for more generous or intelligent co-operation; and no one who is fortunate enough to be able to consult their magnificent volume *Book Types* could doubt that they are among the finest of printers.

## Ancient Mining and Metallurgy Committee: Report on the Examination of a Broken Celt from Scotland

**6** An interesting flat bronze axe was examined on behalf of the National Museum of Antiquities of Scotland. We are indebted to the Keeper, Mr. Robert A. K. Stevenson, for the following note:

The hoard of seven flat bronze axes, from which was taken the sample reported on below, was found in 1947 at the foot of the Hill of Finglenny, far up a glen in the parish of Rhynie, Aberdeenshire (*Proc. Soc. Ant. Scot.*, Vol. LXXXII, p. 292). Three of the axes had been snapped in half before being hidden. This curious feature is shared by three out of seven axes in a hoard from Colleonard, Banffshire, and probably by at least one out of another hoard of seven in the same county. Since broken flat axes are not otherwise common, the question arose whether the objects had been deliberately destroyed, in which case it might seem that the hoards had been deposited as part of a religious or magical act. Though the examination which Dr. Voce so kindly undertook does not provide conclusive evidence, it is of considerable interest.

The Report by Dr. Voce of the Copper Development Association follows.

## REPORT

*Micro Specimen No. 65, Analysis No. 3*

Of the seven celts found together in Aberdeenshire, three were broken in half when found, and it is desired to ascertain whether or not such breakage was deliberate, having some religious or magical significance.

The following is the result of the spectrographic examination of the broken celt submitted:

Tin	present	Lead	not detected
Arsenic	probably about 0.5 per cent.	Zinc	not detected
Iron	trace	Aluminium	not detected

This analysis shows the material to be a bronze containing probably about 8 per cent. of tin, and metallographic examination revealed a homogeneous solid solution comprising twinned equiaxial grains, as shown in fig. 1. The distribution of porosity and inclusions indicated that the article was originally cast and that it had not been greatly, if at all, shaped by forging. On the other hand, the recrystallized structure and the entire absence both of coring and of any tin-rich phase prove that it had been heated fairly strongly. Marked local differences in grain size show, however, that the temperature to which it was raised was far from uniform. This heat treatment, which would tend to toughen the material, may have been applied when the celt was made.

Considerable superficial corrosion of an intercrystalline but not deeply penetrative character had taken place, and this obliterated



much of the evidence which might otherwise have been gained as to the nature of the fracture. The corrosive attack tended to spread along the crystallographic planes, particularly near the fracture where slip bands were more prevalent than elsewhere. The corrosion made it impossible to say with certainty whether the actual fracture had been inter- or trans-crystalline. The final



FIG. 1. MICROPHOTOGRAPH OF BROKEN CELT FROM SCOTLAND ( $\times 75$ )

The figure shows twinned crystals of annealed a bronze with inclusions and porosity. By courtesy of the British Non-Ferrous Metals Research Association

stage of the fracture had been effected by bending the already broken parts outwards to an angle of roughly  $30^\circ$ , and this had caused considerable distortion of the metal near one edge of the specimen. A sample taken from this region showed distortion of the crystals, with strongly marked slip bands, indicating that this final part of the breakage occurred after and neither during nor before exposure to the elevated temperature. While this does not rule out the possibility that the earlier part of the breakage was effected while the metal was hot, the presence of slip bands in crystals near the fracture throughout its length suggests that it occurred while cold.

It would not be very easy to break the cold implement, but this might be done by striking it alternately on opposite sides with another celt while bridged between two supports near its ends. No evidence of such hammering could be found on the specimen examined. It may be significant, however, that a dark band is visible across the middle, and another fainter band near the blade, of one of the undamaged celts included in a photograph of the hoard in question.

The available evidence suggests, but fails to prove, that the celt was deliberately broken. Beyond question the final part of the fracture was intentional, but this may have been no more than the spoiling of an already damaged and useless implement.

It would be an interesting experiment to attempt to break one of the undamaged celts in the manner suggested. A modern bronze of comparable composition and structure would bend considerably before breaking, but impurities and casting imperfections would probably combine to make the ancient bronze more brittle.

E. VOCE

## SHORTER NOTE

### Discovery of the Mandible of a Palaeanthropus at Monte Circeo. Communicated by Professor Sergio Sergi, University of Rome

On 30 August, 1950, Dr. Antonio Ascenzi, an assistant in the Institute of Pathological Anatomy, University of Rome, and Mr. Giovanni Lacchi, a schoolteacher of Albano Laziale, were making an excursion in the neighbourhood of the prehistoric cave of Villa Guattari at Monte Circeo in Lazio. There they discovered a large portion of the left half of the body of a fossil human mandible which lay almost entirely embedded in the steep rock outside the cave. This is the same cave where in 1939 were found a skull (Circeo I) and a mandible (Circeo II), both

from *Palaeanthropus neanderthalensis* (see MAN, 1948, 75 and 91).

On the following day this interesting fossil piece was brought to Professor Sergi, Director of the Anthropological Institute at Rome and on 1 September Professor Sergi and Professor A. C. Blanc investigated the discovery site, with the aim of recovering also the remaining portion of the same mandible which the two discoverers had noticed fully embedded in the rock. The second fossil piece was safely recovered and given to Professor Sergi. Both of these important fossil remains of *Palaeanthropus* (Circeo III) will be subjected to detailed study by Professor Sergi in co-operation with Dr. Ascenzi. The preparation of the pieces will be very difficult on account of the hard rocky incrustations.

## REVIEWS

### PHYSICAL ANTHROPOLOGY

**Races.** By C. S. Coon, S. M. Garn and J. B. Birdsell. Springfield, Ill. (Thomas), 1950. Pp. 153. Price 21s. 6d.

In common with many present-day evolutionary biologists, some physical anthropologists now suggest that the differences in the morphology, physiology and behaviour of the different groups of mankind have arisen as a result of selection and isolation acting on heritable variability. The present book elaborates this thesis and is the first of a series of 'written lectures' which are designed to provide expert discussions of the 'main facts and most important conclusions' of physical anthropology.

The opening chapter contains a dissertation on the biological significance of 'race' and mention is made of the modern tendency to use combinations of all observable characters to separate human races. The next section is devoted to the origins of heritable

variability, the mechanics of natural selection and the evolutionary significance of man's social and cultural systems.

The main part of the book consists of discussions of how various physical features of the different groups of mankind may confer selective advantages in the areas in which they occur. For instance it is suggested that as the 'globular' form of some Arctic peoples provides a relatively small surface area compared to the body volume, it helps to restrict the rate of heat loss. Similarly it is claimed that other features of body build and the pigmentation of the eyes and skin in different groups of mankind may also have an adaptive significance. In the present stage of our knowledge of comparative human physiology, much of this section is necessarily speculative—a fact of which Dr. Coon and his co-authors are fully aware. Some characters may in fact be selectively neutral or even disadvantageous



but are retained because they are genetically correlated with others which have a positive selective value. Others, such as the presence of supraorbital tori, which are discussed in a later chapter of the book, may be merely survivals from earlier phases of human evolution and may or may not have a selective significance at present, although they could have done so at an earlier stage. Other features may have arisen as a result of genetic drift. Unfortunately the elaborate treatment of adaptive characters in the centre section of this book may convey a false impression of the number of characters which are strictly adaptive to the present environment in which a group is living.

In conclusion there are brief descriptions and a list of thirty extant races of men.

The authors have produced a stimulating and very readable book. Their work focuses attention on the fact that, as our knowledge of comparative human biology is extended, it may well be possible to explain man's evolution in terms of the same general principles which are now usually considered to be widely applicable to the rest of the animal world.

E. H. ASHTON

**Sacropelvmetry.** By Bruno Oettinger. *Rev. Mex. de estud. antrop.*, Vol. XI. (Mexico, D.F., 1950), pp. 27-77

9 This short, if verbose, monograph proposes a scheme for the metrical examination of the innominate bones and sacrum. For this purpose, a number of new measuring terminals are defined and named, and the dimensions obtained from them are described, while a number of accepted standard measurements and indices are retained; for example, Martin's Sacral Length-Breadth Index is reproduced in the name of 'Length-breadth: Superior index (ventrad)', and its conventional subdivisions quoted. A note indicating the source of each such reproduction would have been of value.

The object of the study is the 'planimetric outline reconstruction of the two bones under discussion, by means of their exact measurements, regardless of deviations from any plane of projection'; the resulting diagrams are superposable and thus comparable.

A more favourable impression of its usefulness would have been received if the procedures described had been tested by their systematic application to an adequate series of specimens.

D. F. ROBERTS

## ARCHAEOLOGY

**Les Fouilles Préhistoriques (Technique et Méthodes).** By A. Leroi-Gourhan. Paris (Picard), 1950. Pp. 88, 10 diagrams

10 The Director of the Centre for Prehistoric Research at the Musée de l'Homme has produced a short manual which sets an entirely new standard among works devoted to archaeological method in France. The book is written for an informed, but not specialist, audience, and wisely attempts to formulate the general principles upon which excavation should be based, rather than to give minute instructions on procedure. The emphasis throughout is, as the title suggests, upon the examination of palaeolithic sites, chiefly caves. Although work on such sites is outside the experience of the majority of British prehistorians, the author has many suggestions on matters of principle and technique which deserve wider application in this country. In particular, his treatment of the 'natural' constituents of an archaeological deposit, the soils, rocks, animal and vegetable remains, comes as a welcome and necessary reminder of the value of environmental evidence; such material is still too often regarded as of minor importance compared with the recording and interpretation of specifically human artifacts.

The latter part of the book contains a valuable appendix on methods of recording and preserving items of palaeolithic art; a brief section with bibliography on scientific methods of detection and analysis in archaeology, contributed by Madame Annette Laming; a critical bibliography of French, English and American manuals of excavation; and lists of the Regional Directors of prehistoric and historic antiquities in France.

The final section consists of ten annotated diagrams illustrating practical methods of excavation and recording. This is the least satisfactory part of the book, if only because so condensed a treatment may give the inexperienced reader an over-simplified view of the difficulties of excavation. Some of the methods of surveying described, though doubtless of value in the restricted area of a cave, are unnecessarily laborious for use in the open; and, in my experience, the *grattoir* which is the French excavator's standard hand tool may with advantage be exchanged for a trowel on most post-palaeolithic sites. Such minor points in no way detract, however, from the value of this useful book.

R. J. C. ATKINSON

## MATERIAL CULTURE

**Die Entstehung der öffentlichen Finanzwirtschaft.** By Wilhelm Gerloff. Frankfurt (Klostermann), 1948. Pp. 67. Price D.M.350

II Professor Gerloff's brochure on the origin of the system of public finance is a very welcome addition to the growing number of monographs on special aspects of the economic systems of primitive peoples. He is the author of a very interesting book on the origin of money which, because it was published during the war when we were not on speaking terms with Germany, is practically unknown in this country, even though its third enlarged edition appeared in our libraries after the war. In that book he sought to refute the popular conception that money had necessarily originated through barter. He laid much stress on considerations of prestige as the motive force that had induced primitive man to choose certain objects which had subsequently developed into money. In the present work, too, Gerloff emphasizes the importance of the prestige factor in the life of primitive peoples. He seeks to trace the origins of public finance to the contributions of tribesmen to their chief to enable the latter to display wealth, to organize public festivities on a large scale, and to give away presents, all for the sake of enhancing his prestige from which his subjects hope to derive reflected glory.

According to the author, the system of public finance may have arisen gradually from such gifts to the tribal authorities, gifts which, though voluntary in form, are in fact virtually compulsory under the prevailing social system. In due course they become compulsory also in form. Occasional levies are gradually replaced

by regular taxes, which are no longer contributed in perishable products but in imperishable objects which may eventually assume the role of currency. In many instances the contributions of tribesmen to the requirements of their chief assume the form of a proportion of the proceeds of tribal hunting, fishing or other enterprise, which the chief retains after dividing the rest among his subjects.

The author traces the rudiments of various forms of revenue that survive in the modern State. He quotes instances of direct and indirect taxes: the former are assessed on the primitive taxpayers in proportion to their capacity as indicated by the size of their crops or livestock; the latter are payable on certain occasions such as the slaughter of animals or the sale of slaves, and include a tax on turnover in goods sold on primitive markets. There are import and export duties in many primitive communities. The primitive State often retains the monopoly of all foreign trade or of trade in certain goods, such as ivory, as a source of revenue. An ingenious revenue-producing device which Gerloff does not mention and which modern Finance Ministers have not yet discovered is the use of special weights or measures which in many instances secure a profit to the primitive Treasury. For example the kings of Dahomey made payments in strings of 1,500 shells which had to be accepted for 2,000.<sup>1</sup>

It is a pity that the author does not draw more attention to the frequent instances of similarity between the public finances of primitive and comparatively modern communities. For example he rightly states that war requirements were an important factor in



the development of the taxation systems of primitive peoples. The same is true, however, of England in the Middle Ages and even for some time after the Renaissance. But for the financial requirements of wars most kings would have been able to live within their own resources for many centuries and there would have been no need for them to impose taxes on their subjects.

Another point of criticism is that the author has failed to draw a sufficiently clear line between public economy and public finance in primitive communities. Admittedly they merge into each other and the dividing line is much less distinct than in modern communities. Nevertheless it might perhaps have been possible to indicate more clearly the point at which the various practices serve the purpose of financing public expenditure instead of being part of the public economy of the communities, aiming not at the financing of public expenses but at the redistribution of goods.

Notwithstanding these minor omissions, the book is undoubtedly an outstanding contribution to a hitherto neglected subject. It has become fashionable to regard the comparative method, under

which material of evidence is collected from the largest possible number of communities, as out of date. Gerloff's work stands as a vindication of that method as against the more fashionable method of basing sweeping conclusions on thorough but one-sided field-work in a single community. The numerous instances which he quotes tend to point in many respects to conflicting conclusions, and, even though he accepts certain of them after sifting the evidence, the possession of a wide range of facts to a considerable degree safeguards his theories from the pitfalls of dogmatism through unwarranted generalization.

PAUL EINZIG

#### Note

<sup>1</sup>And the Asantehene of Ashanti had his own extra-heavy set of brass weights for weighing payments of gold dust, with the same purpose (supposedly because he must find 'dashes' for certain court officials out of the proceeds; see Rattray, *Ashanti*, pp. 304f.).—ED.

## SOCIAL ANTHROPOLOGY

**An Introduction to the History of Sociology.** Edited by Harry Elmer Barnes. Chicago (Univ. of Chicago Press) (London: C.U.P.), 1949. Pp. xvi, 960. Price \$10

**I**<sup>2</sup> This portly volume is complementary to the *Contemporary Social Theory* of the editor and Dr. Howard Becker (New York, 1940). It opens with the editor's chapters on 'Ancient and Medieval Social Philosophy' and 'Social Thought in Early Modern Times' bringing the survey down to the generation of 1850, both very well done. Then come the 'Pioneers of Sociology,' Comte and Spencer, by the editor, Lewis Morgan appropriately by Leslie White, Sumner, Ward, and Gumpowicz, also by the editor. The last-named might equally have prefaced the 'Leading Sociologists in Germanic Countries,' Wundt, Tönnies, Simmel, Von Wiese, Max Weber, Troeltsch, Sombart, Oppenheimer, Alfred Weber, Freyer, Ratzenhofer, Spann, Ludwig Stein. The last is surely more in place in the next section, for his formative years were in Switzerland, his outlook was eclectic, and he alone overlooks the German horizon, which has so disastrously limited the rest of this group, and thereby attains European influence. In this diverse company one misses Bastian.

'Non-Germanic Countries' are represented by Novicow, Kovalevsky, Fouillée, Tarde, Le Bon, Dürkheim, de Greef; an Italian group headed by Pareto; and the 'Lister Ward of Spanish Sociology,' Posada. The omission of Le Play needs explanation, but it is never easy to delimit sociology.

'English Sociology since Spencer' has also some omissions. It begins with Benjamin Kidd and ends with Arnold Toynbee; Leonard Hobhouse stands in a class by himself, well supported however by Patrick Geddes (with his loyal colleague Branford) and Graham Wallas; Briffault and Westermarck plough their lonely furrows, and the latter is unlucky in his spokesman. Smuts is not even in the index, but occurs once in the text. It is characteristic of the amateurish Englishmen outside the older universities, which have taught sociology under other names, that so many who have contributed to our outlook in these matters rank as historians, economists, or anthropologists. In London, on the other hand, there was not room for both Hobhouse and Geddes.

Finally, 'Sociological Theory in America' is represented by Giddings, who might almost rank as a 'pioneer'; Small, W. I. Thomas, Stuckenberg, Ross, Cooley, Ellwood, and Hayes; Sorokin took refuge in Minnesota, and was called to Harvard, but 'even Spengler excels him in historical judgment'—which is poor praise, and Spengler is below the line—and his vast *Social and Cultural Dynamics* 'combines the faults of European and American social science.' A long roll of Latin American writers ends with the Peruvian, Cornejo, but even he 'either failed to grasp his opportunity' for original studies of South American conditions 'or preferred to work after the European pattern of sociology.' The latter is certainly true; he even ignored Latin American predecessors, and there was no reason for including him in this survey, except that he

popularized sociology for Spanish-reading people. And even they have forgotten him.

As a systematic survey, this book is very well done. The editor's own chapters are uniformly excellent, and he has kept a large and varied team in sufficient order within the general design, though there are diversities of arrangement, and uneven quality. It would be easier to use if the *obiter dicta* about writers outside its scope were more uniformly supported by references. The index to persons is inadequate, and the notes irregularly planned. Sometimes they include topics and illustrative extracts which would have been better placed in the text—in what is essentially a book of reference, one should not have to look in more than one place.

The survey is of especial value as an illustration of the fortuitous and personal way in which a new subject of study looms up, and the practical difficulties of introducing it into established academic systems. One remembers how Tübingen found a niche for Anthropology; in a faculty which included Theology and Christology it was found convenient to have some one who knew about the human nature of Christ. Conversely Tylor said, when asked to examine in anthropology, that he had never sat for an examination. And in this book his name only appears thrice. In this book, Ratzenhofer and Sombart probably owe their place to the teaching of A. W. Small at Chicago, and Small to his influence on Harry Elmer Barnes. Small however was fortunate in his opportunity as organizer of a new department in a rapidly growing university in the centre of a new world: but can *Between Eras* 'without exaggeration be compared with Plato's *Republic*? Something of his wide-eyed sympathy animates the work of his pupil.

JOHN L. MYRES

**Personality in Nature, Society, and Culture.** Edited by Clyde Kluckhohn and Henry A. Murray. New York (Knopf), 1949. Pp. 561. Price \$4.50

**I**<sup>3</sup> This book consists of a number of short papers which have been collected from the writings of some forty psychologists, anthropologists and sociologists and assembled to illustrate a common theme. The theme is that of the editors, Professor Clyde Kluckhohn and Dr. Henry Murray, and it is outlined in two introductory chapters, of which the first describes the concept of personality as it is used in this book, and the second the different factors, biological and environmental, which determine the form an individual personality will take. The editors' theoretical approach is also revealed in the section headings of the book. This is important to bear in mind. Since only one of these essays seems to have been written expressly for this work they do not always illustrate effectively, or even adequately, the immediate point at issue. Nevertheless the order of the sections, their titles and the short introductions which precede them do give some idea of the kind of determinants of personality which the editors have in mind. The essays are to be considered as examples of work that has been done, or could be done, to illustrate Kluckhohn's and Murray's



hypotheses rather than as systematic treatments of any one of them.

Personality is here viewed as an organization of emotional attitudes, ideas and behaviour patterns which provide each individual with an escape from the different tensions to which he becomes subject through his life as a member of human society. The emotional habits we develop allow us a means of self-expression; a design for the focus of our ambitions; a way of reducing the conflicts which arise from the number of alternative courses open to us by means of 'scheduling' the day's and week's activities; and finally a compromise which each individual reaches between reality and the goals he has set himself, and again between the demands of his ego and those of his society. It is suggested that the personalities which predominate in a particular culture are the product of the common experiences to which the individuals brought up in the culture are subject, but this point is not emphasized, and in fact it is the infinite varieties of personality type, and the personality changes in an individual's lifetime, that stand out from this introduction rather than the 'basic' or 'modal' personality thought to be associated with each culture.

The editors' theory of personality is one that I am not competent to evaluate. It is based rather on the psychiatrist's view of society as a series of factors which frustrate and limit the individual's impulses and desires, rather than on the anthropologists' concept of human culture as a means of facilitating activities and achievements that would not have been possible for the individual alone. But there are some obviously suggestive points for the anthropologist here, and particularly I think, the claim that the traditional scheduling of activities within periods of hours, days or months becomes an essential part of those habitual reactions which the layman describes loosely as personality. Here is, in fact, the ethnographer's calendar of seasonal and daily activities viewed from the individual rather than the social angle.

The major determinants of personality are classified here on lines that have been sketched by Kluckhohn and Mowrer in an earlier essay ('Culture and Personality: A Conceptual Scheme,' *Amer. Anthropol.*, Vol. XLVI, 1944). They are briefly the constitutional—including genetic—factors, pathological conditions, and sex and age differences affecting personality development; the group determinants, which seem to include all the cultural influences which affect all the individuals in a group or society; the role determinants or those attitudes developed by individuals who have to fill roles as, for instance, men or women, chiefs or commoners, bureaucrats or soldiers; and lastly the situational, or the historical accidents of birth or individual experience which make one child an orphan, another an eldest son, one hungry and another full.

The essay on the constitutional and biological factors affecting personality includes papers on the genetic determinants of the autonomic nervous system and of schizophrenia; and on the correlation between masculine physique or maternity and personality traits. The selection of papers is inevitably rather a random one, but it does at any rate show the range of genetic and physiological influences on emotional attitudes. 'Biological limits to human variation' seems to me to be a less successful section because it has less unity. It includes a short essay by Margaret Mead on human instinct; one by Ruesch on the correlation between class membership and certain illness rates; and an attempt by Charles Morgan to correlate somatypes with seven paths of life in the philosophical sense which some 140 Americans of different sex, age, height and weight have been persuaded to grade from 1 to 7.

The more directly anthropological data appear in the section on group-membership determinants which includes a series of articles of the more familiar culture and personality type. Here are papers by Erikson on the Sioux and Yurok, by Hallowell on the Salteau Indians, and by Dorothy Eggan on Hopi adjustment. They are carefully chosen as illustrating different types of cultural determinant from early upbringing to later economic influences and adaptations to culture-contact situations, but as is natural in a work of this kind the essays cannot give any systematic exposé of the writers' views. The claim of the editors has been, as they explain, to rescue articles that were in danger of being overlooked in periodical form rather than to publish the best or most characteristic work of the author in question.

The section on role determinants seems to me to be incomplete

to the point of being actually misleading. Here the editors deal with the effects on personality of the traditionally determined systems of relationship of the type more commonly called structural in this country. This is to my mind one of the most important fields of determinants to be dealt with since it includes family configurations, the organization of local face-to-face groups, authority structure and types of economic structure. The discussion of this aspect of the question is, however, limited to articles by Talcott Parsons on the age and sex structure of the United States (*Amer. Sociol. Rev.*, Vol. VII, 1942) and another by R. Merton on the bureaucratic personality (*Social Forces*, Vol. XVIII, 1940). One is grateful to have both these articles reproduced in a handy form, but the section gives little idea of the systematic analysis of this type of determinant which a more inclusive view of social structure would lead to and perhaps shows the limitation of the role concept from the comparative point of view.

The second half of the book is given to discussion of the many situational determinants of individual personality, and as such it is mainly based on psycho-analytic concepts. It includes a number of articles that are interesting and stimulating to the layman in this field, and discusses the application of the personality concept to modern problems in essays such as Erikson's on Hitler imagery and the upbringing of German youth, Mead's on democratic character-formation and personality changes in times of social upheaval, and Powdermaker's on the channelling of Negro aggression.

Here, then, are a number of essays of which many are individually stimulating, although most of the separate sections strike one as unsatisfactory since they merely whet the appetite for more systematic treatment. The whole concept behind the book is however a very important one and it has been attacked with great courage and energy. To begin with, the work forms an effective protest against the over-narrow types of cultural determinism of personality that have been common in recent literature. The emphasis on the constitutional side by side with the cultural seems to me to be particularly valuable. I have never been able to understand why students of culture and personality have found it so easy to wipe out the genetic and constitutional determinants of personality or 'basic personality' on the grounds that our knowledge of somatypes or endocrinology or genetics is incomplete, when the same might be said of our information on personality types. It was to my mind straining at a biological gnat and swallowing gaily a psycho-analytical camel!

But, more than this, the book is welcome because it faces honestly a mass of doubts. It is obvious that the more closely scientists of different disciplines work on the comparative study of personality the greater the number of determinants of this elusive and yet evidently recognizable entity that come to light. Inevitably therefore the clear outlines of the 'culture pattern' or the 'basic personality' which made them so stimulating to teacher and fieldworker are doomed to fade out. The flash of illumination that concepts of this type gave us is almost bound, by the logic of circumstances, to be followed by a period of doubt and confusion until work starts at a deeper level or with new concepts. Meditation on this book makes me feel doubtful in fact whether the culture pattern set of ideas can be of much further use at the moment to the anthropologist who is by training and interests looking for uniformities in human conduct, although it can obviously continue to provide stimulus to the psychologist who is studying individual differences in personality and their causes, and hopes to be able to measure these with greater and greater precision. Is it not therefore time for the anthropologists themselves to take the wheel and set out for destinations of their own choosing instead of providing more and more information for the use of the psycho-analyst or the educationist? The uniformities which could, I think, be tackled much more systematically are those that belong definitely to the field of comparative sociology; I mean those which govern the formation and maintenance of values systems and which result in common expressions of emotional attitudes by peoples of different temperament and 'situational determinants' on the conscious level. I believe it will be found that such values systems are as capable of being systematically classified under headings that are universal in any human culture as Malinowski's 'cultural imperatives,' because they are centred like



these in major structural principles, activities and biological needs, that is to say on major or rather inevitable institutions. This book confirms my view that the next contribution to this important type of study is to be made by the anthropologist, and that it consists in the systematic classification of systems of values in different cultures and the use of such a classification as a scheme for the collection of data in the field with the anthropologist's own methods and own tools.

AUDREY I. RICHARDS

**The Origins and History of Religions.** By John Murphy. Manchester (University Press), 1949. Pp. viii, 454. Price 25s.

**14** This is a book mainly for students and the general reader. Though it contains many spontaneous opinions, it is almost devoid of references, though obviously compiled from well-known authorities, industriously and impartially, and with wide range of acquaintance with the subject. The latter part of the book (ch. xv-xl) consists of outline summaries of the principal religious systems, that of the Bantu peoples being used to illustrate the advance from the elementary clan or tribe towards the nation or the empire, and concurrent advance from Animism or Dynamism to notions of a Supreme Being—and of departed relatives. This leads to what the author calls the Civilized Horizon, illustrated in Babylonia, and then to Hebrew religion for which he adopts Sir Leonard Woolley's rationalization of the tribal legend of Abraham. The Hebrew deity was literally the 'God of Abraham, Isaac and Jacob,' the specific deity of an isolated tribe of emigrant nomads. Only after this are we referred to Egypt, the Indo-European group—Greek, Roman, and Indian—and the distinct lines of development in China and Japan, followed by Islam, Zoroastrianism, and the Sikhs, Jains, and Mandaeans. Christianity is not considered at all.

The reasons for this arrangement emerge from the earlier chapters, on religious origins. The method is throughout comparative, though the author disclaims this. Religions are classified as monotheistic, polytheistic, monistic, animistic, and primitive; the last-named tentatively grouped as the *mana* type. But the types overlap; and so too do the 'horizons' of belief which the author himself distinguishes—in reverse order—as the primitive, among food-gatherers and primitive hunters; the animistic, in settled tribal societies and hoe cultivators; the agricultural, with cereals and domestic animals, worship of an Earth-mother, and nature powers, not yet fully anthropomorphized; the civilized with urban communities, conceptions of law, and the development of individuality; and the prophetic where great individuals exert creative and formative influence over large masses of people. This last 'horizon' would seem to correspond with the 'axis period' of Jaspers (*Vom Ursprung und Ziel der Geschichte*, Zurich, 1949).

The disproportionate treatment of the religious experiences of India and of China is explained by the opportunities offered in them for historical analysis and assignment of this or that specific aspect to its proper 'horizon.' Conversely the account of Islam, which had less varied experiences historically, is summary; and the religions of the New World are not considered at all.

The evidence for religious beliefs—or rather of probably religious observances—in prehistoric times, is examined at some length. The arguments of Wilhelm Schmidt for primary monotheism are discussed rather summarily and rejected; the observations of Wilhelm Koppers on Bhils and Yaghans were perhaps not published fully in time to be considered. Other theories of degeneration and reversion—von Eickstedt, Speiser, Immenroth, Wegener, and Porteous—are reviewed, and in part accepted; and modern 'primitives' illustrate the notions of *mana* and its outcome in action. There is a corresponding chapter on 'totemism' as a phase in the consolidation of societies, and another on 'taboo' and tribal custom. Similarly the 'king,' the 'priest,' and the 'prophet' illustrate the emergence of 'civilized' from 'tribal' society, and of discussion and persuasion among the problems arising under the rule of law.

It will be seen that the somewhat unusual arrangement results from much independent thought; and it provokes discussion and reflection at many points. It is less than a formal treatise, but more than a textbook; and deserves a better index to make its *obiter dicta* more accessible.

JOHN L. MYRES

**Primitive Law.** By A. S. Diamond. 2nd ed. London (Watts), 1950. Pp. 451. Price 15s.

**15** This book was first published in 1935, but seems not to have been reviewed in MAN. The present volume is advertised as a new edition, but there is no indication of revision, and the latest date in the bibliography is 1932. The author has made a thorough study of the early written codes, from Hammurabi onward, and of the oral codes of some savages, almost exclusively African Negro. As a result he has set out to disprove some of Maine's contentions, especially his contention that all law was originally religious. Maine wrote, of course, at a time when much less was known of early and savage law than is now, and many fallacies may be detected or suspected in his works. Some of these are successfully exposed by Dr. Diamond, but he himself has adopted other fallacies.

In the first place he assumes that every tribe has advanced to its present stage of pastoralism or agriculture, quite independently of external influences, through a series of earlier stages, and has altered its laws *pari passu*, every type of law being associated with some stage of progress. The fact that cattle-keeping and corn-growing must have been diffused from the limited areas in which the ancestors of these animals and plants were found wild has obviously never come to his notice, and he fails to realize that since we know nothing of the earlier history of savages, the attribution to them of previous stages is purely hypothetical; we cannot be certain that a tribe did not receive its culture ready made from the outside.

Dr. Diamond's belief in independent invention does not stop at African tribes; he holds that the successive empires of the Middle East developed their codes in complete independence of their predecessors, and scorns the idea that Anglo-Saxon laws could possibly have been influenced by the Bible. This fallacy leads him to another, namely that in the earlier codes, or those associated with the simpler cultures, we are near the beginnings of law, and that if, therefore, we do not find in them specific religious sanctions attached to laws, such laws must have had a purely secular origin. But the history of savages, though unknown, is as long as our own, and the beginnings of their law are as far off. The more 'primitive' a law, the older it probably is, and the longer there has been to forget its origin. And though savages may not attach specific sanctions to laws, they commonly believe that the breach of any law or custom will incur the displeasure of the ancestral spirits. This in itself constitutes a religious sanction.

Dr. Diamond tells us, again, that 'the process of legislation is well understood among the tribes' (p. 105). This statement, if not fallacious, is at least exaggerated. Dr. Meek, it is true, says that among the Ibo the senior age grades have the power to legislate, but the subjects he mentions suggest bye-laws rather than laws, and it is improbable that any tribe has ever had the power or the wish to alter its ancestral laws. Consider the *'usûl al-'ashûr'*, the code of tribal laws which is observed by peasants and Bedouin alike throughout the Arab world; it differs considerably from Islamic law and has probably remained unchanged since before the rise of Islam. There is no body of persons who, even if they wished to change it, could possibly do so.

Dr. Diamond has ignored Maine's warning that 'the grand source of mistake in questions of jurisprudence is the impression that those reasons which actuate us in the maintenance of an existing institution have necessarily anything in common with the sentiment in which the institution originated.' He has brought to light, however, striking similarities in the laws of people widely separated in time and space, and his book is, in spite of its defects, a valuable contribution to a neglected branch of social science. RAGLAN

**Adaptation.** Edited by John Romano. Cornell Univ. Press (London, Geoffrey Cumberlege), 1949. Pp. xiv, 113. Price 16s.

**16** This short book is a symposium of talks on adaptation delivered on the occasion of the opening of a hospital's psychiatric wing. The contributors are Professor Paul Weiss, a zoologist, Professor Homer W. Smith, a physiologist, Professor Howard Liddell, a psycho-biologist, Professor Lawrence S. Kubie, a psychiatrist, and Professor Clyde Kluckhohn, an anthropologist.



The editor is Professor John Romano, who holds the chair of psychiatry in the University of Rochester, where the lectures were delivered.

To one who has a mere nodding acquaintance with two of the disciplines from whose point of view adaptation is discussed, and who does not know the meaning of a third (psycho-biology), criticism is rather difficult. Nevertheless, there is obviously much of interest and value to be gleaned by the layman. I found Professor Weiss's analysis of the difference between adaptation, with its implicit assumption of flexible incompleteness, and dead-end specialization most vividly expressed, but felt that Professor Smith, in his eagerness to erect an anti-metaphysical platform, forgot to say anything very much when it was built. Professor Liddell, on the other hand, is refreshingly undogmatic in either the philosophical or the scientific sense. With no more than the most tentative comments, he outlines a series of thought-provoking views on animal conditioning, stating that 'theories of behaviour come and go' and that he merely wished 'to play the role of enthusiastic salesman displaying his wares.'

Professor Kubie's article on the Neurotic Potential and Human Adaptation contains a most valuable statement on his concept of neurosis. He claims that we cannot recognize neurosis by the behaviour which it produces, but only in relation to the total personality of the individual concerned. Thus a person behaving in what is broadly thought of as a healthy manner may just have built up excellent defences around himself and have so arranged his life that the walls are not fissured. Interesting examples are given of this type of neurotic adaptation: for instance, a man who had a severe unrecognized phobia of towns contrived to spend a happy life in the country but fell into a depressive panic when forced to change his habitat. The acid test of a high neurotic potential is whether or not it engenders behaviour which is obsessively repetitive and incapable of change should the defensive ramparts—which to some extent we all possess—be dynamited. Should this happen the neurotic patterns

are shown up clearly, for they endlessly repeat themselves in circumstances to which they are decreasingly apposite. Professor Weiss draws attention to the universality of the neurotic potential, but stresses a fact that is often forgotten—for the word 'neurotic' is still a veiled term of abuse—that these 'same potentialities of the human spirit also make possible our power of abstract thought and feeling.' There is much in this article which is wise and valuable.

Professor Kluckhohn's article is largely a critique of functional anthropology. While admitting his use of a modified functional technique in much of his own work, he regrets that the strict formulation of sociological principles has led to the exclusion of other relevant theories—those, for example, of psychiatry, psychology and learning theory. Drawing on these he defines certain postulates common both to social anthropology and to these other disciplines. These, he claims, widen the field to such an extent that we lack, as yet, the 'conceptual instruments' to do more than define highly tentative hypotheses about it. In some societies, he admits, 'culture change may well be predetermined once the culture has assumed its fundamental organization. Much more, however, culture change seems to be due to the ceaseless feed-back between factors of idiosyncratic and universal human motivation, on the one hand, and factors of universal and special situation, on the other.' Despite what Professor Kluckhohn seems to think of English anthropology, there are no doubt many anthropologists in this country who would agree with this point of view, though not necessarily with the reasons which he gives for holding it. These reasons, however, are given with a sensitive subtlety which claims for them careful consideration.

Most of these articles, though excellent in themselves, are naturally on the slight side, and will do little to increase the knowledge of the specialist in the field with which they are concerned. Nevertheless, apart from its intrinsic value, this book presents the English reader with an interesting cross-section of sophisticated American thought on an important topic.

ADAM CURLE

## AMERICA

**Maya Hieroglyphic Writing: An Introduction.** By J. Eric S. Thompson. Carnegie Institution of Washington, Publ. 389. Washington, D.C., 1950. Pp. 347, frontispiece and 64 figs. Price \$7 paper, \$7.50 cloth

Knowledge of the Maya has so greatly increased since Bowditch and Morley wrote that there is now need of an up-to-date and authoritative treatment of the subject. The present work admirably fulfils that need.

The author is very careful to give credit to other investigators. His analysis of Goodman's real discoveries and also of his unsubstantiated claims is illuminating. One may add that though the word 'Determinants' is due to Teeple, yet the idea is due to Bowditch who clearly stated it in 1910. At least two of his examples have been accepted by Thompson elsewhere.

There is a full discussion of the principles of Maya writing, much of the material for which was discovered by Thompson himself. I agree that more phonetic elements were used in Maya writing than in that of the Aztec, but I am strongly of opinion that the nature of the two writings was the same. It still remains true, as Forstemann said long ago, that the Maya never attained to the expression of a verb; therefore their script was not a true system of writing, but only an embryo one. Nor did they know how to weigh anything, for in the whole of pre-Columbian America only the Peruvians had

much to be said for Roy's doubts whether chronicles in the Mexican or European sense existed in pre-Spanish Yucatan. Thompson rightly states that the Long Count was a count of *tuns*, not days, that there is nothing to show why 4 Ahau 8 Cumku was taken as a starting point, and that possibly the Maya did not consider that their chronology had any definite beginning. Nevertheless the problem still remains to be solved of why they decided on 4 Ahau 8 Cumku.

I agree that when the Long Count was first thought of there was probably no greater time unit than the *baktun* and that the perfecting of the calendar passed through several stages, the 260-day period being earlier than any other. He rejects my suggestion made in 1924 as to its origin, but the Quiche today (Schultze-Jena, 1946) say that 260 days are equivalent to 9 lunar months and that this period is the duration of pregnancy. I think that this conclusively proves my opinion and that the 13 gods and 9 gods of Thompson were only arrived at later as derivatives from the 260-day period. After the 260-day period with its 13 market months of 20 days each had come into general use, it would be quite natural to use 360 days (the *tun*) as an approximate year, just as we often refer to a month as 4 weeks. Later than this there followed the 365-day year and later still the calendar round.

The author also treats very fully of the ending



the Dresden Codex indicate Mexican influence and so may help to date the Codex. Probably the interest in Venus was due to fear of its rays rather than admiration for its beauty. The same motive applied to their eclipse tables. Thompson's new method of reading Yucatan dates is a valuable discovery. I agree that the pictures in the divinatory almanacs are subordinate to the glyphs. His whole treatment of this is very able. There is an excellent appendix on the almanacs of the Books of Chilam Balam and an interesting chapter on calculations into past and future.

Altogether this book is a great and important contribution to its subject.

RICHARD C. E. LONG

**El Valle y la Ciudad de Mexico en 1550.** By S. Linné. *Ethnog. Mus. of Sweden, N.S., Pub. No. 9. Stockholm, 1948. Pp. 220*

**18** The University Library at Uppsala possesses a pictorial sketch map of the city of Mexico and its surroundings, painted in colour on parchment about the middle of the sixteenth century. It is the earliest surviving plan of the city built by the Spaniards after Tenochtitlan had been razed to the ground, and is therefore of particular importance. The central part is more or less to scale, at about 1/10,000, but the margins are much distorted. Buildings are conventionalized, but the area outside the town is enlivened by numerous figures and groups of people, mainly Indians, travelling along the paths, cutting wood, hunting, or fishing in the lake, but, curiously enough, not cultivating the fields.

Reproductions have been published before, notably by Nordenskiöld, Dahlgren and Maudslay, but these were imperfect in various respects. The volume under review contains an unretouched coloured reproduction, slightly reduced, in a pocket at the end, in which the best modern methods have been used. Apart from this, it consists of three parts; the first contains introductory chapters on the discovery and conquest of Mexico; and on the Aztec town; the second is a pleasantly discursive account of the valley in the sixteenth century, in the form of a series of excursions on the map in and around the town; the third contains speculations on the authorship of the map, how it reached Sweden and its subsequent history, to which is added a useful account of the fluctuations and draining of the lakes since the Conquest. Finally there is a series of key maps, free drawings of the original divided into eleven sections, on which the excursions of the second part may, with some patience, be followed.

The first part adds little if anything to what was already known, but it is necessary to set the stage. The second part is the longest, and in following the author in his leisurely walks and rides round the town and valley the reader cannot fail to learn much about Mexico in the early days of the Colony, while a number of digressions on methods of hunting and fowling will be of special interest to the ethnologist. In the third part, it is concluded that the map was drawn by an Indian educated by the Spaniards, perhaps in the College of Santa Cruz at Tlatelolco, before the floods of 1555. It is at any rate clear that the style of drawing is not indigenous and that the author was to some extent familiar with Mexican hieroglyphic writing. It is suggested that the map was looted from Prague in 1648 by Swedish mercenaries, which may be true, but the statement in the book, that there are strong reasons for thinking so, is an exaggeration.

The volume is beautifully produced, with fine photographs. One of its main objects is to further the collaboration, already fruitful, between Mexican and Swedish anthropologists, and for this reason it is in Spanish. Minor misprints occur but are rare, and only two are of any importance. On p. xiii of the introduction 'el Este ha sido colocado en lugar del Norte' should surely read 'el Oeste . . .', and on p. 204 the date 1451 is probably meant for 1541.

G. H. S. BUSHNELL

**Nomads of the Long Bow: The Siriono of Eastern Bolivia.** By Allan R. Holmberg. *Smithsonian Institution, Inst. Soc. Anthropol., Publ. No. 10. Washington D.C., 1950. Pp. iv, 104, 7 plates. Price 65 cents*

**19** That part of Bolivia through which the Siriono Indians roam is bounded mainly by the Mamoré and San Martín Rivers, and to the south by a line traversing Santa Cruz. Attempts have been made for

25 years by Catholic missionaries, and since the thirties by the Bolivian authorities, to gather together various groups of this tribe and try to persuade them, through missions and so-called *Escuelas Indigenales*, to give up their former nomadic way of living and turn to a method of living based on agriculture—an experiment which, according to the latest reports, does not seem to have entirely succeeded.

The North American ethnographer Allan Holmberg has taken advantage of the favourable opportunity thus presented for a thorough study of the Siriono Indians, partly at the Government station of Casarabe, and partly by staying some length of time among a group whose method of living is as yet uninfluenced. Anyone who is at all acquainted with the problems involved in an undertaking of this kind must give him full recognition for the manner in which he performed his work.

Holmberg considers hunger anxiety to be the dominating reason for the primitiveness of the Siriono Indians. From his observations most of the native's time is spent on the quest for food or resting from it; under such conditions little seems to remain for the pursuit of other activities. The society is not equipped with cultural techniques for dealing with its environments so as to offer surety of food supply. Agriculture is but little developed; weapons are cumbersome; tools are almost lacking; and food is not stored or preserved in any abundance or for any length of time.

Holmberg's data on the material culture of the Siriono are largely a repetition of previously known facts; the greater merits of the treatise lie in its non-material aspects. The relation of the Siriono culture to that of the mound-builders, which appears in many parts of the same district, is not touched upon. Nor does he share the theory held by Nordenskiöld that the Siriono represent a substratum of culture which once existed widely in the area they now occupy, but holds rather that they have gradually been pushed northward into the sparsely inhabited forests they now live in, and that in the course of their migrations they have lost much of their original culture. An acculturation caused by the lack of ability to acclimatize their mode of living to new surroundings with entirely new living conditions would thus be the basic cause of the hunger drive having become the dominating factor in the society of the Siriono.

STIG RYDÉN

**Pedigrees of Negro Families.** By R. Ruggles Gates. *Philadelphia (Blakiston), 1949. Pp. vii, 267. Price \$5.50*

**20** The two hundred or so pedigrees described in this book, mainly of American Negro families, were collected by students of genetics, in many cases among their own families and friends, and were checked for reliability by the author. Many of the pedigrees are fairly extensive, covering four or five generations, and contain a generous number of offspring. The conditions dealt with include many physical and mental characteristics and some diseases, with a mode of inheritance well known in white races. Other conditions are included in which the importance of genetic factors is in doubt, although the author does not always make this clear. There is an interesting chapter on the inheritance of skin colour, in which the author proposes his own hypothesis of the mechanism involved. A brief mention only is made of the distribution and inheritance of blood groups and there is no account of any inborn errors of metabolism in Negroes.

In those cases where the number of pedigrees is large enough to enable the mode of inheritance to be determined, it is found to be qualitatively the same in Negroes as in whites, although there may be quantitative differences in gene frequencies. This is the main, and not unexpected, conclusion drawn by the author.

The pedigrees are accompanied by descriptive material, with occasional elementary accounts of the genetical principles involved. The writing, on the whole, is clear, but there is a tendency to burden the text with irrelevant clinical details. Moreover, in the chapter dealing with abnormal mental conditions, much of the explanatory matter abounds in technical psychiatric and medical jargon and must be meaningless to most of the readers for whom the book is meant.

The book is well produced and the diagrams are very clearly set out.

M. LUBRAN



## ASIA

**Lushai Chrysalis.** By A. G. McCall. London (Luzac), 1949. Pp. 320, illus. Price 25s.

**21** Though the author, who was in charge of the Lushai Hills District of Assam for 12 years, writes primarily as an administrator his book demands the attention of anthropologists interested in the effects of culture contact. Shakespeare's *Lushei-Kuki Clans* and Parry's *Monograph on Lushai Customs* are chiefly concerned with the 'untouched' Lushai. Major McCall's theme, on the other hand, is the material and, especially, psychological effects of fifty years of administration and of missionary activity so intense that animism is, outwardly, practically dead. A background of indigenous Lushai custom is sketched in, and the documented instances of certain practices are valuable, but it is not always clear what customs survive and what have died out. The chapter on 'The Bawi Controversy' is, I think, the first full published account of the abolition, under mission pressure, of the retainer system. This so-called reform not only created a new class of mendicants but seriously undermined the position of the chiefs, who are still the main agents of the Government. An examination follows of the position of the Government *vis à vis* the missions. Here the author, with his long experience and interest in psychology, is at his best. When Major McCall wrote—and the position is probably the same today—the mission maintained a staff of 20 Europeans, all normally settled for long years, compared with at most three administrative officers all liable to transfer; all education was in the hands of the missions, who were by far the largest employers of the products of their schools; in almost every village there was a salaried pastor, whereas the Government was represented only by unpaid Chiefs. The result was that while the Government officers were responsible for law and order the real power lay with the missions, to whom the new and vocal black-coated class looked for careers. The missions too had their difficulties, for whereas they were themselves effectively controlled by distant Boards in Europe, they had great difficulty in supervising their many teachers scattered over an area without communications and each the local embodiment of 'higher culture.' For the problems arising from the situation the book must be consulted. Solutions are examined, but the political situation in India has so completely changed since it was written that the relevance of much of this section has inevitably diminished.

There is a full synopsis, but the lack of an index is a defect. The proof-reading leaves much to be desired; for instance on p. 96 *gaurus* becomes *gaveous*. J. P. MILLS

**Criminal Law and Procedure in Nepal a Century Ago.** By Leonhard Adam (from notes left by Brian H. Hodgson). *Far Eastern Quart.*, Vol. IX, No. 2 (1950). Pp. 23.

**22** This interesting little pamphlet consists of brief memoranda, hitherto unpublished, left by that remarkable scholar and diplomat Brian Houghton Hodgson, with a longer introduction and notes by Dr. Leonhard Adam, of the University of Melbourne, who knows well the country to which the memoranda relate. Hodgson was Assistant Resident and then Resident in Nepal from 1820 to 1843. Little escaped his observation. Nepal, an independent native state in the Himalayas, still possessed in the early part of the nineteenth century a backward system of law representing much the same stage of development as the law of England in the thirteenth century. In his articles entitled 'Some Account of the Systems of Law and Police as Recognized in the State of Nepal' (1834), Hodgson gave us a valuable contemporary account of the

law of the country. For it he drew in part on the present memoranda, which are merely lists of gaol deliveries in 1826, 1829, 1833 and 1843, that is to say records of punishments for crime. Such precise data are worth much theory. The capital offences were mainly murder, incest with blood relatives or mother-in-law, intercourse with a Brahman woman by a man of lower caste, other serious offences against persons of high rank, and offences against cows (killing a cow, or bestiality). Then there are the offences punished by mutilations, especially the cutting-off of a hand, finger or ear for theft of a more or less aggravated character or for coining, the cutting-off of genitalia for sexual intercourse with a Brahmanee or other person of high rank or with an outcaste, and the cutting-off of the nose of a woman committing or procuring adultery. Banishment is the sanction for serious offences committed by Brahmans, for they could not be killed or mutilated. The lesser offences (adultery with a married woman, petty theft, assaults and slight wounding, or serious false accusations) are punishable by imprisonment or a fine. A. S. DIAMOND

**Twilight in India.** By Gervé Baronté. New York (Philosophical Library), 1949. Pp. xix, 382, 11 plates. Price \$3.75.

**23** There are good, bad and indifferent books on India, but this book is not only inept and tedious but mischievous and dangerous. It must infuriate self-respecting Indians and confuse and poison the mind of any undiscerning Westerner who happens to read it before going to India. While Katherine Mayo's one-sided presentation was redeemed by the reformer's passionate anxiety to lessen suffering and expose cruelty, there is no excuse for the vituperations and ignorant generalizations of this book, which regrettably appears in a respectable series among the works of eminent scholars. A review in MAN would be out of place were it not that journals of learned societies have sometimes the obligation to denounce the pseudo-science of writers claiming a knowledge which they do not possess. The book is so full of errors of fact and misinterpretations that one wonders how anyone could live in India and remain so remote from the life of her people. Sentences such as 'All meat eaters, including the Europeans, are "Untouchables"' reveal an ignorance which is too blatant to call for detailed comment, but what should not pass unchallenged is the author's plagiaristic practice of taking, without any acknowledgement, whole sentences from such works as Thurston's *Castes and Tribes of Southern India* and passing them off as her own. Lines 28-33 on p. 78 and 1-5 on p. 79, for instance, can be found *verbatim* in Vol. I, p. 102 and 103, of Thurston's work published in 1909. Similarly the whole of p. 49 and lines 1-10 on p. 50 are verbally taken from Vol. I, pp. 273-5, of that work. It is indeed difficult to understand why the publishers went to the expense of reprinting in a comparatively costly edition disjointed extracts from a publication which is neither rare nor difficult to obtain. C. VON FÜRER-HAIMENDORF

**The Coorg Tribes and Castes.** By L. A. Krishna Iyer. Madras (Gordon Press: agents, Hind, Kitab, Ltd.), 1948. Pp. 74. Price Rs.7/8/-

**24** This little book is based on the notes left by the author's father, the late Dr. L. K. Ananthakrishna Iyer, who undertook a tour of Coorg in 1934, collecting ethnographic data. I regret very much to say that it is superficial, and abounds in errors and misstatements as to fact. I hope neither the author nor his distinguished father will be judged by this book, as both of them have made contributions of undoubted value to Indian ethnography. M. N. SRINIVAS

## EUROPE

**Classical Impulses in Scandinavian Art from the Migration Period to the Viking Age.** By Haakon Shetelig. *Instituttet for Sammenlignende Kulturforskning, Series A, Vol. XIX.* Oslo (Aschehoug), 1949. Pp. 151, 60 figs.

**25** In a lecture course Dr. Shetelig traces the various external influences upon Scandinavian art, and also the expression of

indigenous feeling from the fourth to the tenth century A.D. In the fourth century Germanic peoples met a Scythian-Hellenistic tradition near the Black Sea and provincial Roman work near the Danube. Finds from Thorsbjerg show added pictures of animals so naive and barbaric that they hardly hint at Oriental or Roman influences. In the latter half of the fourth century and the beginning



of the fifth, popular Roman chip carvings became transferred to German metal work. Old connexions with the Black Sea area were cut, but Scandinavian links with Gaul continued. Coin-like trinkets of gold attempt to imitate Roman work but are barbaric. After about A.D. 500 a Roman frontier no longer existed and barbarian kingdoms were trying to develop in West and Central Europe. Women's brooches become a leading form of art work in Scandinavia with indigenous expression strongly developed. By A.D. 600 the Germanic peoples had conquered much land to the south and west of this earlier home and they had learned band-plaiting as a decoration. From about A.D. 800 one can speak of Viking art and the author discusses the Oseberg treasure in special detail. The belief in magic expresses itself in animal heads designed to frighten away evil influences, but there is a rich development of patterns with Carolingian relationship. These few notes give but a feeble idea of a discussion revealing insight and deep knowledge.

H. J. FLEURE

**Life in a Welsh Countryside.** By Alwyn D. Rees. Cardiff (Univ. of Wales Press), 1950. Pp. 188, 12 plates, 35 figs. Price 12s. 6d.

**26** The merits of studying intensively a selected area as representative of a whole is clearly demonstrated in this vivid account of life in the Welsh borderland parish of Llanfihangel yng Ngwynfa, Montgomeryshire. In this strip of country, sloping from high moorland at over 1400 feet in a complex series of valleys down to the Vymwy river at 600 feet, lived in 1939-40, when the basic field work was carried out, some five hundred people, about as many as one fieldworker can get to know well in two seasons.

A comparative description of the use of space by these people, their farms and fields, the layout of the farmyard and the interior arrangements of the house, with many diagrams and a wealth of critically evaluated historical material, precedes the social analysis. In this way the local study area is placed correctly in its space relations with neighbouring parishes, with other parts of the country, and its individual peculiarities are recognized. Although the layout of the book follows in a general way the Le Play nomenclature, family budget material is excluded, an omission not fully justified by the statement that 'from an economic point of view, the countryman has continued to live in a world of his own, the standards of which differ from those of our modern industrial civilization' (p. 30).

The social ties that link these people together are followed with care and insight in chapters on the family, the kindred, the neighbourhood, religious life, status systems and politics. The distinctions that emerge between the Welsh countryside and the English village are stressed but not unduly. The 'hearth of the lonely farm itself is the social centre. The farms are not outlying members of a nucleated community . . . and their integration into social groups depends on

the direct relationships between [the people living on them]. The traditional social unit . . . is *tefn gwlad*, the neighbourhood in the countryside' (p. 100). Strongly conscious of kin and neighbourhood, difficulties arise for these people in making adjustment to urban living where linking ties are often fragmentary and depend on 'interests' rather than blood; these difficulties are not lessened by their adherence to a religious organization 'of a kind that cannot be crystallized in institutions that would ensure maximum continuity.'

Mr. Rees has produced a valuable addition to our growing knowledge of the traditional rural society and it is to be hoped that he will be encouraged to apply his technique either to further studies in this field or to a study of Welsh urban life. J. M. MOGEY

**Les Origines de la Civilisation polonaise: Préhistoire, Protohistoire.** By Jozef Kozłowski. Paris (Presses Universitaires de France), 1949. Pp. 671, 261 illus.

**27** This French translation of a book published in Polish in 1947 aims at giving a complete picture of the early Polish culture of the period from the sixth to the end of the eleventh century A.D., embracing the Polish 'Dark Ages' up to the middle of the tenth century and the beginning of Poland's historical past. The book is divided into three uneven parts. The first and largest, containing 331 pages out of 507, is devoted to the description of the material culture as it emerges out of the huge archaeological material collected on several of the earliest Polish strongholds (*grody*) and boroughs excavated by the author himself and other Polish and German scholars. It includes a detailed study of every-day utensils, weapons, personal ornaments, etc. Great attention is paid to the building craft and timber-earth constructions of fortifications which show amazing convergences with those of the Lusatian culture of the first half of the first century B.C. The second part, containing 111 pages, deals with the customs and spiritual life of early Poland. It is based mainly on the works of several eminent Polish historians and ethnographers and to some extent on the evaluation of archaeological remains; in the nature of things it leaves many problems unsolved. This applies even more to the third part, of 50 pages, which aims at describing the social structure of the early Polish tribes and of the early Polish state; it is entirely based on linguistic speculations and on some historical data collected and expounded by several Polish historians, linguists, and sociologists.

Professor Kozłowski's book is a first attempt to present the culture of early Poland in its full and true aspect. The work is well grounded upon the available archaeological material and upon the results of other disciplines and it corrects many inaccurate views repeated by some scholars concerning the level and the origins of this culture. No historian interested in the cultural development of Middle Age Europe can pass over this work. T. SULIMIRSKI

## CORRESPONDENCE

**U.N.E.S.C.O. on Race.** Cf. MAN, 1950, 220

**28** CHER MONSIEUR.—'Qui veut trop prouver ne prouve rien.' Ce vieux et toujours vrai dicton est sans doute le principal reproche que l'on doit adresser au manifeste de l'Unesco. Le but déclaré de celui-ci, but que tout anthropologiste ne saurait qu'approuver, était de montrer que le racisme, thèse politique néfaste, n'avait aucune base anthropologique réelle, que les arguments scientifiques sur lesquels il prétendait s'appuyer étaient faux ou tout au moins reposaient sur une déformation—volontaire ou involontaire—tendancieuse de ce que révèle l'observation des faits. Après les tragiques événements des dernières années, une telle démonstration avait besoin d'être faite et d'être rendue publique. On ne saurait donc s'élever contre l'idée directrice du manifeste. On doit par contre regretter l'argumentation qui y a été présentée.

On a l'impression que, pour enlever toute base anthropologique au racisme, les auteurs du manifeste ont pensé qu'il était nécessaire

de réduire au minimum, voire de supprimer tout à fait, le concept anthropologique de race, de faire de celle-ci, comme ils le disent explicitement, 'non tant un phénomène biologique qu'un mythe social.' Or l'existence de la race chez l'Homme est un fait biologique incontestable. Le nier, ou même le minimiser, dans l'intention, si louable soit-elle, de supprimer ainsi toute base au racisme dépasse le but. Celui qui, à tort ou à raison, ne croit pas au socialisme, se croit-il obligé de commencer par nier l'existence de la société? Celui qui attaque le nationalisme va-t-il d'abord nier l'existence des nations?

Certes, sur une question aussi importante que celle de la race, la rédaction d'un texte qui ne soulève aucune critique est à peu près impossible. Du moins était-il désirable que les affirmations présentées à un public avide de connaître la vérité ne risquent pas d'être considérées comme aussi tendancieuses dans leur sens, que ne l'étaient, dans un sens strictement opposé, celles qu'avançaient les tenants du racisme!



Laissant de côté certaines affirmations touchant la génétique et sujettes à discussions, je considérerai les trois principaux points en rapport avec les tendances générales indiquées ci-dessus.

1. *Efforts visibles pour minimiser la race.*—La race est donnée (par. 4) comme perçue différemment par chaque groupe. Il est indiqué que 'nos observations sont affectées par nos préjugés,' que 'nous sommes enclins à interpréter arbitrairement et inexactement toute variabilité . . .'. Or, c'est contre une telle façon de voir que s'est toujours élevée l'anthropologie. C'est pour établir objectivement les différences entre races qu'elle a créé une méthode précise, et il est injuste de ne pas mentionner ses efforts en ce sens et les très importants résultats concrets auxquels elle est déjà parvenue.

Dans le même esprit, le manifeste laisse entendre (par. 8) que les anthropologistes sont incapables de classer l'humanité au-delà de la séparation en les trois groupes, Blancs, Jaunes et Noirs. Une telle déclaration ne tient pas compte de nombreux faits solidement acquis: si les classifications raciales ont changé, c'est qu'elles se perfectionnent à mesure que s'accroissent nos connaissances des groupes humains; l'existence des races secondaires (si on appelle races primaires les trois groupes précédents) n'en est pas moins un fait certain. La classification des Insectes, telle qu'elle est admise aujourd'hui, est totalement différente de celle qui était classique il y a seulement 30 ans: dira-t-on pour cela que les entomologistes doivent renoncer à classer les Insectes?

Pour donner plus de force à la déclaration précédente, le manifeste, quelques lignes plus loin, spécifie que la plupart des groupes ethniques (terme substitué à celui de race) 'n'ont pas encore été étudiés, ni décrits.' Ceci était vrai il y a un siècle. C'est ignorer ou vouloir déprécier l'anthropologie que d'écrire une telle phrase en 1950.

Poussant à l'extrême la tendance minimisatrice de la race, le manifeste déclare finalement (par. 14): 'la race est moins un phénomène biologique qu'un mythe social.' Une telle affirmation, qui en arrive à mettre en cause l'existence même de la race chez l'Homme, est en contradiction non seulement avec les faits, mais avec tous les paragraphes antérieurs du manifeste! Elle est grave de conséquences. Elle ne sera certainement approuvée par aucun de ceux qui s'adonnent à l'anthropologie physique.

2. *Efforts pour prouver que les aptitudes mentales sont identiques dans toutes les races.*—Le manifeste touche ici à un très grave problème et qui préoccupe depuis longtemps les anthropologistes. Partant d'arguments de sentiment ou de quelques faits inexactement interprétés, le racisme a voulu faire des généralisations audacieuses. Le manifeste donne l'impression de tomber dans l'excès contraire en tirant des conclusions d'ensemble d'expériences encore très rares et limitées à quelques groupes.

Les affirmations émises à propos des tests psychologiques (par. 9), des différences de tempérament (par. 11), de personnalité et de caractère (par. 12) entre groupes humains, laissent entendre que toutes les recherches scientifiques déjà faites auraient montré qu'il y a, à ce point de vue, identité de comportement entre tous les groupes humains. Cette conclusion, d'abord présentée comme probable, se transforme en une quasi-certitude à la fin du manifeste: 'les recherches scientifiques révèlent que le niveau des aptitudes mentales est à peu près le même dans tous les groupes ethniques' (par. 15, no. 2). Comment, dira le lecteur, une telle affirmation peut-elle se concilier avec la phrase citée plus haut et qui déclare que la plupart des races 'n'ont encore été ni étudiées ni décrites' (par. 8)? Or, si cette phrase est inexacte en ce qui concerne les caractères physiques, elle garde justement toute sa valeur pour les caractères mentaux.

Il est possible que la proposition du paragraphe 15 soit vraie. Mais actuellement les recherches sont beaucoup trop rares, elles ont porté sur un nombre trop minime de séries et de races, pour que la vraie réponse scientifique soit autre que celle-ci: 'Ignoramus.'

3. *Confusion créée autour du terme race.*—Pour enlever au racisme jusqu'au mot même aux dépens duquel il a été formé, le manifeste propose de remplacer le terme 'race' par celui de 'groupe ethnique.' Croit-on que supprimer le mot va supprimer la chose? Et veut-on ressusciter la vieille querelle médiévale des nominalistes et des réalistes? Depuis plus de 100 ans, les anthropologistes s'efforcent d'éviter la confusion entre la race, fait biologique, et le groupe ethnique, fait culturel, confusion incessamment renouvelée par les trop nombreux laïques qui estiment avoir le droit de s'occuper

d'anthropologie sans vouloir s'astreindre à l'étudier d'abord. C'est peut-être un des points sur lequel tous les anthropologistes sont le plus unanimes que de réserver le mot 'race' aux seuls groupes humains basés sur les caractères physiques, d'accord en cela avec ce qui a lieu en zoologie et en botanique. Substituer à ce terme celui de 'groupe ethnique' est un remède pire que le mal et qui va exactement à l'encontre du but proposé. Il désorientera les lecteurs du manifeste. Il est inacceptable pour les anthropologistes.

Le manifeste de l'Unesco mérite donc de sérieuses réserves. Il comporte des contradictions, des affirmations trop catégoriques, des négations mal justifiées. Puis-je dire enfin qu'il paraît pécher par excès d'optimisme. 'L'instinct de coopération . . . a des racines plus profondes que n'importe quelle tendance égocentrique' (par. 14); 'les recherches biologiques viennent étayer l'éthique de la fraternité universelle; car l'Homme est, par tendance innée, porté à la coopération . . .' (par. terminal). Plût au Ciel que ces paroles aient exprimé des vérités indiscutables. Car, s'il en avait été ainsi, il n'y aurait jamais eu de racisme, et il n'y aurait pas eu besoin non plus de manifeste. . . .

Musée de l'Homme, Paris

Dr. H. V. VALLOIS

29 STR.—The interesting statement prepared for and issued by U.N.E.S.C.O. to clarify its attitude towards questions of race could have been improved, but is nevertheless of considerable value. Most students would agree that very few populations show racial homogeneity. A couple of Mashona men standing side by side and belonging to the same tribe showed that one might have been a Dinka or a Shilluk from the Upper Nile with his long limbs and spare build, while the other man was much like many a Hottentot. It is therefore not very scientific to give, for example, average cephalic indices for various regions and to base even political claims on these. One is glad that the U.N.E.S.C.O. document takes a firm stand against this nonsense.

Many would agree, as a practical measure, to the division of mankind into Negroid, Mongoloid and Caucasian, as the document circulated indicates, but there is a danger here. The layman may think that, at some early period, humanity split in this way and that the three have been more or less separate since. In other words, the layman might think this classification genetic. This could be disputed. It seems at least probable that there was an early dispersal in several directions of populations which, though not homogeneous, included a considerable proportion of extreme long-heads with certain accompanying characteristics. Some of these have become incorporated among each of the three practical divisions and these divisions thus relate to certain genes and not to others.

The statement rightly quotes from Darwin's *Descent of Man* that, in process of time, small groups have been integrated into larger communities and the unity of mankind is indicated as the goal of social endeavour. The promotion of this spirit of co-operation is one of the greatest of modern needs, and we shall approach the task best if we realize that we are trying to diminish the old feeling against 'the stranger.' We cannot presume the existence of the tendency to universal co-operation in mankind at large; we have to try to build up this universalism bit by bit. The process has been going on for many centuries, and needs acceleration.

Edinburgh

H. J. FLEURE

30 STR.—Far from being regarded as 'established scientific facts,' the conclusions of the 'experts' all appear to be controversial questions. The conclusions are merely the misguided opinions of a particular school of anthropologists whose assertions appear to be motivated by wishful thinking.

I take their main points one by one, in the order in which they are summarized in the press release.<sup>1</sup>

(1) Racial 'discrimination' needs some definition. If discrimination signifies capability of scientific differentiation and definition, then the mere fact that the human species has evolved into diversified and descriptively definable races is a flat contradiction of the statement. And in biology the term discrimination has no other meaning.

(2) That range of mental capabilities is 'much the same' in all



racess is scarcely a scientifically accurate statement. It is at most a vague generalization. It is, however, scarcely true, for temperamental and other mental differences are well known to be correlated with physical differences. I need but mention the well-known musical attributes of the Negroids and the mathematical ability of some Indian races. Of course, if we wish to assess marks for such attainments to each race, it is conceivable that all races may end up with the same total; but there is no scientific proof that this would be the case; our knowledge is far too meagre for any such categorical statement. Even if it were true that there is 'no proof that the groups of mankind differ in intelligence, temperament or other innate mental characteristics,' it is certainly the case that there is no proof of the contrary.

(3) Racial interbreeding may or may not be a good thing. In specific instances crossing may, on genetic grounds, produce good results; on the other hand, it may not always be advantageous. Nature, in evolving different racial types, was endeavouring to produce the best fitted each to its own environment. On the whole a good job seems to have been made of it; why interfere? A categorical denial that ill may result seems premature in the present state of knowledge.

(4) That race is more than a social myth is endorsed by the plain facts of physical anthropology and ethnography. Denial of facts is no way to prevent intertribal or interracial strife. These, as Keith has well shown, are factors in evolution and we cannot close our eyes to them, however unpleasant they may be.

(5) There is no point here with which I am in serious disagreement. I agree that, in the past, much confusion has arisen from erroneous synonymization of race with language, religion and other social usages.

(6) The points raised here are adequately covered under (2) above.

(7) I have no serious objection here.

W. C. OSMAN HILL,  
*Professor*

*Zoological Society of London*

#### Note

<sup>1</sup> This press release of 18 July, 1950, the opening and closing passages of which have already been quoted in MAN (1950, 220), is of course the form in which the U.N.E.S.C.O. statement is most widely known to the general public. In view of Dr. Osman Hill's letter, it seems desirable to reproduce here the summary of conclusions which was contained in it (and which was published in *The Times* of 18 July):

(1) Racial discrimination has no scientific foundation in biological fact.

(2) The range of mental capacities in all races is much the same. There is no proof that the groups of mankind differ in intelligence, temperament or other innate mental characteristics.

(3) Extensive study yields no evidence that race mixture produces biologically bad results. The social results of race mixtures are to be traced to social factors. There is no biological justification for prohibiting inter-marriage between persons of different ethnic groups.

(4) Race is less a biological fact than a social myth. As a myth it has in recent years taken a heavy toll in human lives and suffering and still keeps millions of persons from normal development, and civilization from the full use of the co-operation of productive minds.

(5) But, scientifically, no large modern national or religious group is a race. Nor are people who speak a single language, or live in a single geographical area, or share in a single cultural community necessarily a race.

(6) Tests have shown essential similarity in mental characters among all human racial groups. Given similar degrees of cultural opportunity to realize their potentialities, the average achievement of the members of each ethnic group is about the same.

(7) All human beings possess educability and adaptability, the traits which more than all others have permitted the development of men's mental capacities.

It should in fairness be pointed out that this summary is, if

anything, less carefully worded, especially under (4), than the statement itself.—ED.

31 SIR,—I consider that the U.N.E.S.C.O. statement on Race quoted in the October number is somewhat unsatisfactory. Certain of its statements and conclusions suggest a philosophical or ideological doctrine rather than a 'modern scientific' one. For example, what evidence is there for any scientific belief that man is born with biological drives towards universal brotherhood and co-operation and how is it adduced? To find such proof in 'the growth and organization of man's communities' is merely to revive an out-moded psychological theory of instincts. The opposite conclusion, namely, that man lacks a biological drive towards universal brotherhood, etc., would be equally plausible in the light of alternative evidence of overt human behaviour.

Again, I cannot agree that it would be helpful to substitute the phrase 'ethnic group' for the concept of 'race' because of popular fallacies about the meaning of the latter. To me this suggests almost a species of magical technique based on the idea that something awkward or troublesome can be got rid of by the mere process of calling it by another name. We do not solve a slump in trade by calling it a 'recession' and I doubt if many white Southerners in the United States or supporters of Dr. Malin in the Union would feel less racially prejudiced towards Negroes and Africans even if we all agreed to stop referring to the latter in racial terms. My own view is that it is precisely by continuing to instruct the public in the proper anthropological use and meaning of 'race' as a group concept that we can best clear up the confusion over what is culturally acquired and what is genetical. To use the phrase 'ethnic group,' when the intended reference is specifically to the physical characteristics rather than the way of life of a people, will only increase misunderstanding.

Whilst I commend most heartily the essential thesis of the document I feel that in these and in general respects its authors have relied unduly on moral and educational effect. If the object is to combat racial prejudice, it is not sufficient to limit attention to superstitious thinking about racial mixture, racial superiority, etc. These are merely the rationalizations of prejudice. It is necessary, also, to my mind, to make quite clear to the public that racial prejudice is rooted deeply in the social structure and that, so far as can be judged from historical and contemporary evidence, it is associated with some fairly specific types of social organization. In other words, it should be emphasized that racial prejudice is a function rather than the cause of situations of group conflict.

*Department of Social Anthropology,  
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K. L. LITTLE

#### Note

32 It is probable that most anthropologists everywhere, hearing of a proposal to empanel the world's leading authorities on race to meet in Paris and produce a statement of the established scientific facts on the subject, would think first, and without a moment's hesitation, of Professor Vallois, as one to whom there could be no more obvious choice; and the absence of any reference to his name was one of the most extraordinary features of the Ashley Montagu Statement published by U.N.E.S.C.O. on 18 July, 1950 (reproduced in full in MAN, 1950, 220). His views (expressed in a manner, moderate yet decisive, for which the French language is perhaps a more perfect vehicle than any other) are therefore particularly welcome.

The Hon. Editor is glad to report the receipt of a letter addressed to him as Hon. Secretary of the Royal Anthropological Institute by Dr. Alfred Métraux, who became Head of the Division for the Study of Race Problems at U.N.E.S.C.O. after the Statement had been prepared, but shortly before its publication. This letter announces, as 'one of the first results of the article "U.N.E.S.C.O. on Race" which appeared in MAN' (1950, 220), a decision by the Director-General to convene in 1950 a panel of physical anthropologists and biologists (including British representatives) to write a new statement which would not be open to the criticisms to which the existing statement has given rise. This decision to reconsider the matter in the first place from the physical aspect (and later, it is to be presumed, from the



point of view of social anthropology, which has not been in issue), together with the promptness with which it has been taken, reflects great credit upon the Director-General (Dr. Jaime Torres Bodet); at the time of going to press, it is not clear what effect it will have upon the distribution and publication of the Ashley Montagu Statement. U.N.E.S.C.O. is in a unique position to assist in the preparation and dissemination of a truly definitive statement of established facts on race, but such an *ex cathedra* statement to the world must clearly be such as to command the assent of all recognized authorities.

These columns are now open for the discussion, on a more positive plane, of the extent and content of a definitive statement; contributions will be welcomed from Great Britain and elsewhere, and the hope is expressed that they may be of value in the deliberations of the new panel.—ED.

**Social Anthropology: Past and Present.** Cf. MAN, 1950, 198, 254 and 271

33 SIR,—I find myself in happy agreement with all of Professor Evans-Pritchard's main contentions. I believe that, other things being equal, understanding of a society or culture will always be deeper and more significant if its past is known than if it remains unknown or disregarded, just as the understanding will gain depth and meaning in proportion as it is set in a larger and larger context. I agree that the anthropologist's phenomena, especially cultural ones, are highly refractory to the finding of laws and causes but do increasingly yield worthwhile patterns and meanings if we will look for them. The long and successful career of linguistics serves as an indication that such is the case; for language can after all be viewed as but a special part of culture. The proper aim of anthropology seems not so much generalized abstraction as individuation. Instead of resolving phenomena away by transferring them from their natural setting into an artificial one of rationalization or experiment, anthropology aims to preserve the identity of its phenomena—even to heighten it intellectually by relating it to other individuated identities. The relation to historiography is patent: sensitivity, empathy, articulateness are definite aids in both. Though these are aesthetic qualities, neither anthropology nor historiography would seem ultimately classifiable as an art, because the arts create previously non-existent patterns, whereas historical studies uncover existing ones.

Much of anthropology unquestionably falls within the humanities, by the usual classification, though it is to be hoped that anthropology will never feel called upon, like so much of classical studies, to propagandize or apologize for certain values as superior to others. I hesitate only at Professor Evans-Pritchard's finding that we are studying societies and their cultures as 'moral' and not as 'natural' systems—unless 'moral' means value-bearing and 'natural' means mechanical. There seems no reason why patterns and their values should not be regarded as parts of nature and studied as parts of the history of nature. History, in its widest sense, would appear to be basically a manner of approach of studying nature, rather than something contrasting with nature. I concede that this construction leaves 'social science' as a rather ill defined residue between the humanities and the sciences dealing with subhuman phenomena; and I note that Professor Evans-Pritchard does not place social science.

I share his lack of conviction as to applied anthropology, but realize that mine rests on temperamental mildness of interest, as compared with the zest of intellectual or aesthetic recognition of patterns and values. I agree also that the attempt to find satisfactory explanations of cultural phenomena through psychology is a 'building on shifting sands'—although psychological aspects are always present and important in culture.

Columbia University, New York

A. L. KROEBER

34 SIR,—It does not appear to me that the majority of theoretical anthropologists in this country would subscribe to functional theory as it is expounded by Professor Evans-Pritchard in his Marett Lecture. In particular, I suggest that the modern functionalist is not opposed to history, but only to historicism; that is, to the view that an understanding of modern social institutions may be obtained only through an examination of

their origins. In common with scientists in general, the anthropologists hold that the aim of science is explanation. Again, in common with other scientists, the anthropologist uses, though usually implicitly, a theoretical scheme of a hypothetico-deductive character, of the kind analysed by Professor Popper in his *Logik der Forschung*. Theory, in this view, consists of a body of logically interconnected series of hypotheses which enable us to make predictions about the real world. These predictions, the logical consequences following from the hypotheses, are then confronted with the empirical evidence at our disposal. If they are not falsified, our explanations may be said to have been 'confirmed,' although they can never be 'proven' in any ultimate sense. In this way, a consistent body of knowledge emerges gradually as the result of 'trial and error.'

This brings me to my second point. The modern historian uses essentially the same method as does the scientist. The difference is that whereas the scientist focuses his attention on the discovery of 'general laws,' the historian is primarily interested in what Professor Popper calls 'independent evidence,' that is, the explanation of particular events. It is a difference of interest rather than of method.

It may be, however, that Professor Evans-Pritchard subscribes to a different view of historical method. In classing history with subjects such as art and philosophy, he appears, on the surface, to support certain 'schools' which claim that historical knowledge may not be obtained by ordinary scientific analysis, but only by various more or less 'intuitive' methods. This point is crucial. For the vital distinction to be made is not between the generalizing and the particularizing sciences, but between science on the one hand and propositions which fall outside the realm of science on the other. Propositions based upon intuitive perception alone differ from those of science in that they are not capable of being tested. This implies that knowledge of this sort is not accessible to all, but only to those whose intuitions lead them to similar results. It is vital, however, that all who wish to test the validity of our propositions should be able to do so, for only in this way, by rejecting some hypotheses and accepting others, can science make progress. It is this 'interpersonal' aspect which gives science at once its dynamic and its objective character.

Would Professor Evans-Pritchard really have us substitute 'private sense' for the common sense of science?

London

A. T. CAREY

35 SIR,—Professor Forde's formula (MAN, 1950, 254) is impressive, but as he regrettably gives no specific example of its application, we must, if we wish to test his theory, take one for ourselves. If we call ortho-cousin marriage X and Islam Y, we can apply the formula, the reason being of course that the Arabs have diffused with Islam a pre-Islamic custom. It is clear, however, that this connotation (if that is the right word) has no affinity with the laws of physical science.

Usk, Monmouthshire

RAGLAN

**Malinowski's Views on Magic and Science.** Cf. MAN, 1950, 238

36 SIR,—According to Lord Raglan, Professor Childe, by showing that magical practices are associated with the craft of metallurgy, in which prehistoric man had a high degree of technical mastery, has demonstrated the error of Malinowski's statement 'that magic appears where knowledge fails.' It is apparently assumed that Malinowski believed in an inverse correlation between magic and technical mastery. This is a quite inaccurate interpretation of his views. What he described as the sphere 'where knowledge fails' was that of 'the incalculable and unpredictable factors,' as Firth has called them, which enter into almost any productive activity, however great the technical mastery. Professor Childe's data reinforce this view.

London School of Economics

LUCY MAIR

**Comments on the Dentition of the Fossil Australopithecinae**

37 SIR,—In a recent paper by Ashton and Zuckerman on some quantitative dental characters in apes and man (*Phil. Trans. Roy. Soc. B*, Vol. CCXXXIV, 1950, pp. 485ff.), a number of conclusions are presented which, it seems to me, may be



misleading to those not personally acquainted with this field of study. I refer particularly to the statements that the teeth of the Australopithecine dentition are no different in size, shape and proportion from those of modern apes. Without entering on a detailed discussion of this question, I should like to draw attention to the following points.

1. The measurements employed are only the over-all dimensions of height, length and breadth. They can therefore only provide data for a very limited number of *general* indices. This, of course, raises the question how far such indices are of any value in determining relationships between different taxonomic groups (even though they may be of assistance in differentiating varieties or subspecies in a single group). Thus, it would not be difficult to find molars of similar height, length and breadth in different families of primates, ungulates, carnivores, etc., but these could not be taken to connote a relationship in the taxonomic sense. It is true that in the past such indices have been used as *accessory* evidence in the study of fossil hominoids, but I think most anatomists have come to realize that they are really not of great value in this respect unless they are considered in relation to morphological details.

2. The authors repeatedly state, on the basis of these over-all measurements, that this or that tooth does not differ in 'size and shape' from one of the modern apes. But these far-reaching statements are based on not more than three major dimensions; in some cases, indeed, they are actually based on only two. Thus the anterior lower premolar of *Paranthropus* is stated not to deviate in either 'size or shape' from the orang, and a reference to the data shows that only the length and breadth of the tooth have been measured. Clearly the terms 'size and shape' are not legitimate in this reference, for an almost indefinite number of quite different shapes (and also different sizes) may, of course, display identical over-all dimensions. In the discussion of their results, Ashton and Zuckerman reprimand a number of authorities rather severely for basing conclusions on inadequate biometrical data. But, in making such comprehensive statements on shape and size on the basis of two dimensions only, are not the authors committing just this very error? It may perhaps be argued that the context makes it clear that, in referring to 'shape and size,' the authors only mean to indicate an over-all proportion such as that represented by the length-breadth index. But I suggest that this explanation does not really justify the use of these terms, particularly since Professor Zuckerman has also stated elsewhere (out of context) that certain of the teeth of the Australopithecine do not differ in shape and size from those of apes (*Nature*, Vol. CLXVI, 1950, p. 158).

3. In their metrical analysis of the deciduous teeth of *Australopithecus*, Ashton and Zuckerman do not make comparison with the milk teeth of man. They make the remarkable statement that 'In no single dimension or index of its deciduous teeth does *A. africanus* differ markedly from the existing great apes.' Because of its unfortunate ambiguity, this statement may be very misleading to the reader who does not realize that, in the case of the lower canine and first milk molar for example, it is based on one index only in each tooth. The authors do not point out that, on the same criterion of such inadequate statistical data, it would be equally possible to state that in no single dimension or index of the deciduous teeth does *Homo* differ markedly from the anthropoid apes.

4. That the shape of the canines and anterior lower premolars of the permanent dentition and the lower canines and first molars of the deciduous dentition are in fact very different from those of apes can be readily demonstrated by direct comparisons and also by measurements. But, naturally, the latter must in the first instance be selected to test the obvious contrasts which present themselves as the result of direct visual comparison. As only one example of what appears to me to be the rational application of metrical analysis to such problems, certain dimensions of the lower milk canines may be mentioned. In every specimen in a series of 44 young chimpanzee skulls, the tooth is conical, and the max. a.p. length of the upper half of the tooth is thus less than that of the base at the level of the cingulum; the percentage ratio shows a range of 56 to 72, with a mean of 63.3, and a standard deviation of 4.3. In *Paranthropus* and *Australopithecus* the milk canine is spatulate as in modern man, and the ratios are 116 and 126 respectively. Such an obvious difference

would not, of course, be revealed by routine indices based simply on over-all measurements.

5. If the authors' statements that (for example) the canine and anterior lower premolars in the Australopithecine do not differ in size and shape from those of modern apes were justified (using the terms 'size' and 'shape' in their proper connotation), it should be possible without difficulty to produce for inspection numbers of teeth of modern apes demonstrating visually this alleged identity, or at least something very closely approximating to it. I can only say that I myself have not been able to find such specimens in a systematic examination of nearly 300 ape skulls, and I would ask the authors whether they can do so in the series which they themselves have studied. It seems to me that theoretical conclusions drawn from such limited statistical data always need to be verified by the production of actual specimens in this way, for this provides one obvious method of testing the validity of the methods employed.

6. The authors, on the basis of comparison with ancient Egyptians and Australian aborigines, state a conclusion that orang teeth resemble those of man more closely than do those of the Australopithecine. But the term 'man' in this connexion is surely not permissible, for it must be taken to include not only *Homo sapiens*, but also extinct types of man (such as Neanderthal man, Heidelberg man and the *Pithecanthropus* group) in which the teeth have very different dimensions. It seems likely that when the teeth of the *Pithecanthropus* group are studied metricaly (and details of some of these are still in course of publication), differences between the dentition of primitive man and that of the Australopithecine will not be very obvious.

7. There is reason to suppose that, through neglect to have measurements checked on the original specimens according to their own specifications, those of the fossil teeth on which the authors base their conclusions are not wholly accurate. For example, the maximum bucco-lingual diameter of the milk canine of the Taungs skull is given as 7.1 mm., a figure which an examination of a cast would have shown to be impossible. It has evidently been taken from one of Dr. Broom's monographs where it is obviously a typographical error; the actual dimension of the left, undamaged, canine (kindly checked for me by Dr. L. H. Wells on the original specimen) is 5.1 mm. It is on the basis of their erroneous measurement that the authors conclude that the breadth-length ratio of the lower deciduous canine of *Australopithecus africanus* does not differ from the chimpanzee and gorilla and is even significantly greater than the orang.

8. Under the heading of *Proconsul africanus* the authors include a number of teeth which were provisionally allocated to this species by Dr. MacInnes in a preliminary report published in 1943. But it is now known, from systematic studies based on much more complete material, that these teeth belong to at least three different species (ranging in size from apes a good deal smaller than the chimpanzee to others as large as a gorilla). They also include several teeth which are actually referable to a different genus. The conclusions of the authors as regards *Proconsul africanus* are thus not necessarily valid.

9. Lastly, I would like to emphasize (as I have already done on previous occasions) that in the assessment of taxonomic affinities reliance should probably never be placed on one or two isolated characters. The total morphological pattern presented by a series of characters in a particular combination should rather be used as the criterion. For example, in the Australopithecine skull there are a certain number of characters which I have not been able to match in a series of ape skulls examined for this purpose. It is possible, of course, that further investigation may show that they do occasionally occur individually in an ape's skull, but the important question is whether they ever occur in the same combination. As another example, statistical data indicate that the probability of finding a chimpanzee lower milk canine with a ratio of basal length to the antero-posterior length of the upper half equal to (or even closely approximating) that of *Paranthropus* is very remote indeed. But the probability of finding such a ratio in a chimpanzee in combination with the other hominid characters (even though they are not all entirely independent)—such as the convex posterior border of the



crown, the absence of a talonid at the base of the crown, the breadth-length ratio of the upper half of the tooth, the contact between the anterior and posterior borders of the crown above the level of the cingulum with the lateral incisor in front and the milk molar behind, the relatively low height of the unworn or moderately worn canine in relation to the height of the first and second milk molars, etc.—is negligible. Finally, the chances of finding several such specimens at random in a common group of apes (to parallel the consistency with which a similar combination is found in the several Australopithecine specimens now known), and of finding these in association with the other combinations of hominid features which have been recorded in the rest of the dentition (deciduous and permanent), in the skull, and in the limb skeleton, may be ignored altogether. Thus it must be accepted as certain that, in the totality of their morphological characters, the Australopithecine resemble man much more closely than do any of the anthropoid apes.

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#### Note

Professor Le Gros Clark's letter has been shown to Professor Zuckerman, whose reply follows.—ED.

**38** SIR,—If the impression gained by other readers of the memoir published by Mr. Ashton and myself is the same as Professor Le Gros Clark's, then I do not doubt that they will be seriously misled. I sincerely hope, however, that this will not happen. The purpose of our work is clearly stated. It was to define, by modern biometrical and statistical methods, the major dimensions of the teeth of living apes (*Phil. Trans. Roy. Soc. B.*, Vol. CCXXXIV, 1950, pp. 471ff.)—a task which to the best of our knowledge had not been undertaken before—and to compare the results obtained with the dimensions of the teeth of several fossil Primates, as 'published by the workers responsible for the description of the fossils' (*ibid.*, pp. 485ff.). In the case of the African specimens we have therefore confined ourselves to figures mainly given by Broom, Dart, Hopwood and MacInnes. Within their context there is no ambiguity about any of our statements, nor have we implied anywhere that our conclusions relate to considerations other than those given numerically in our Tables. When we refer to the length of a fossil tooth, we refer to a dimension that had previously been used by some author to back a statement of comparison. When we refer to the shape of a tooth, we state explicitly which shape index we have used. Within this context our conclusions stand. In the part of our work which concerns the Australopithecine, we quote some thirty specific statements about the dimensions of the fossil teeth in relation to those of existing apes. Examination shows that practically all need to be qualified.

May I make the following observations about Professor Le Gros Clark's separate points?

1. Our purpose was to discover whether statements about relative differences in the magnitude of dental dimensions were or were not correct. We have never claimed anywhere that the dimensions of height, length and breadth, and the general indices of tooth shape derived from them, determine taxonomic relationships. Whatever most anatomists may believe about the overall dimensions of teeth, we were, I believe, the first to question published statements about the proportions of the Australopithecine teeth in relation to those of living apes that have been supported by such metrical information alone.

2. Professor Le Gros Clark is under a misapprehension. All our statements about size or shape refer specifically to some published observations about relative size whose accuracy we have examined. For this our biometric data were fully adequate.

3. The dimensions of the deciduous teeth of *A. africanus*, which we quoted, and which we found did not differ significantly from existing apes, were those on the basis of which the authors cited had made contrary claims. Our statistical analysis shows that they were in error. Only test will show whether Professor Le Gros Clark is right in his predictions about the relative overall size of the human milk teeth.

4. The new facts which Professor Le Gros Clark gives are

interesting. They do not, however, relate to the statements whose accuracy he has questioned.

5. While again quoting out of context, Professor Le Gros Clark implies here that he does not appreciate the purpose for which statistical methods are used in the comparison of the dimensions of structures in the body. No one will at any time be studying more than a sample of the total population of, let us say, gorillas or chimpanzees. A main reason for using statistics in biometric studies is to express variation in such a way that one can determine whether a central trend in one group, e.g. a given dimension of a tooth, diverges to a statistically significant extent from a corresponding trend in another. Statistical techniques are not necessarily verified in the way he suggests. Their design takes account of the size of sample studied.

6. The term 'man' certainly applies to ten groups we have investigated. I agree, however, with Professor Le Gros Clark that extinct types of man should also be studied. We have, in fact, made an analysis of the dimensions of the teeth of *Pithecanthropus pekinesis* (*Sinanthropus*).

7. I should be grateful if Professor Le Gros Clark would point out other inaccuracies, in the monographs we cited, in the dimensions of the fossil teeth. We have computed a new overall index for the lower milk canine, which we have compared, together with the new dimension of 5.1 mm., with the corresponding measures in the chimpanzee. The two do not differ significantly at a level of  $P < 0.02$ .

8. Conclusions about *Proconsul africanus* may not necessarily be valid. What stands, however, is our comparison of dental dimensions, irrespective of the species or genus to which the teeth may be attributed, with the corresponding dimensions in the living great apes.

9. I have commented elsewhere on Professor Le Gros Clark's observations on 'total morphological patterns' (*Nature*, Vol. CLXVI, 1950, p. 158). On this point I completely agree with him—given that the constituents of the pattern are accurately stated. Moreover, in the monograph which has stimulated Professor Le Gros Clark's letter, we explicitly say that there may be attributes of the Australopithecine teeth that are better matched in the human than in the ape jaw. On the other hand, the paper was not concerned to discuss whether the Australopithecine resemble man more closely than do any of the living great apes—a belief which is at least more modest than the recurrent theme, in the monographs from which we quoted, that the Australopithecine are closer to man than they are to anthropoid apes.

In conclusion, may I say, contrary to Professor Le Gros Clark's opening statement, that we do not claim anywhere in our monograph that 'the teeth of the Australopithecine dentition are no different in size, shape and proportion from those of modern apes.' May I quote from our summing-up? 'Apart from showing,' we wrote, 'that accounts of the conclusions previously drawn from assessments of the size and shape of the fossil teeth considered in the present paper need qualification, our analysis does little more than point to the fact that in their metrical attributes (i.e. the metrical attributes which formed the basis of our comparisons) these teeth are more ape-like than human.'

S. ZUCKERMAN

Department of Anatomy, University of Birmingham

**On Whiteness.** Cf. MAN, 1950, 16, 193 and 255

**39** SIR,—The Azande are a somewhat light-coloured race, varying, as Professor Evans-Pritchard says, 'from chocolate to light reddish-brown.' The soldiers of the old government in the Bahr-el-ghazal, though led by Europeans such as Gessi Pasha, were mostly no lighter, so that if they were described as 'white' it must have been on account of their clothes and not their complexions. Light-coloured men no doubt visited many parts of Negro Africa at various times, but, whether or not they were described by the natives as 'white,' they could not have become traditional figures because tradition is concerned with ritual and never with history. Mr. Wainwright's culture hero was probably a ritual figure akin to the king of Omundri in Nigeria, who, according to Dr. M. D. W. Jeffreys, was at his installation whitened all over with clay.

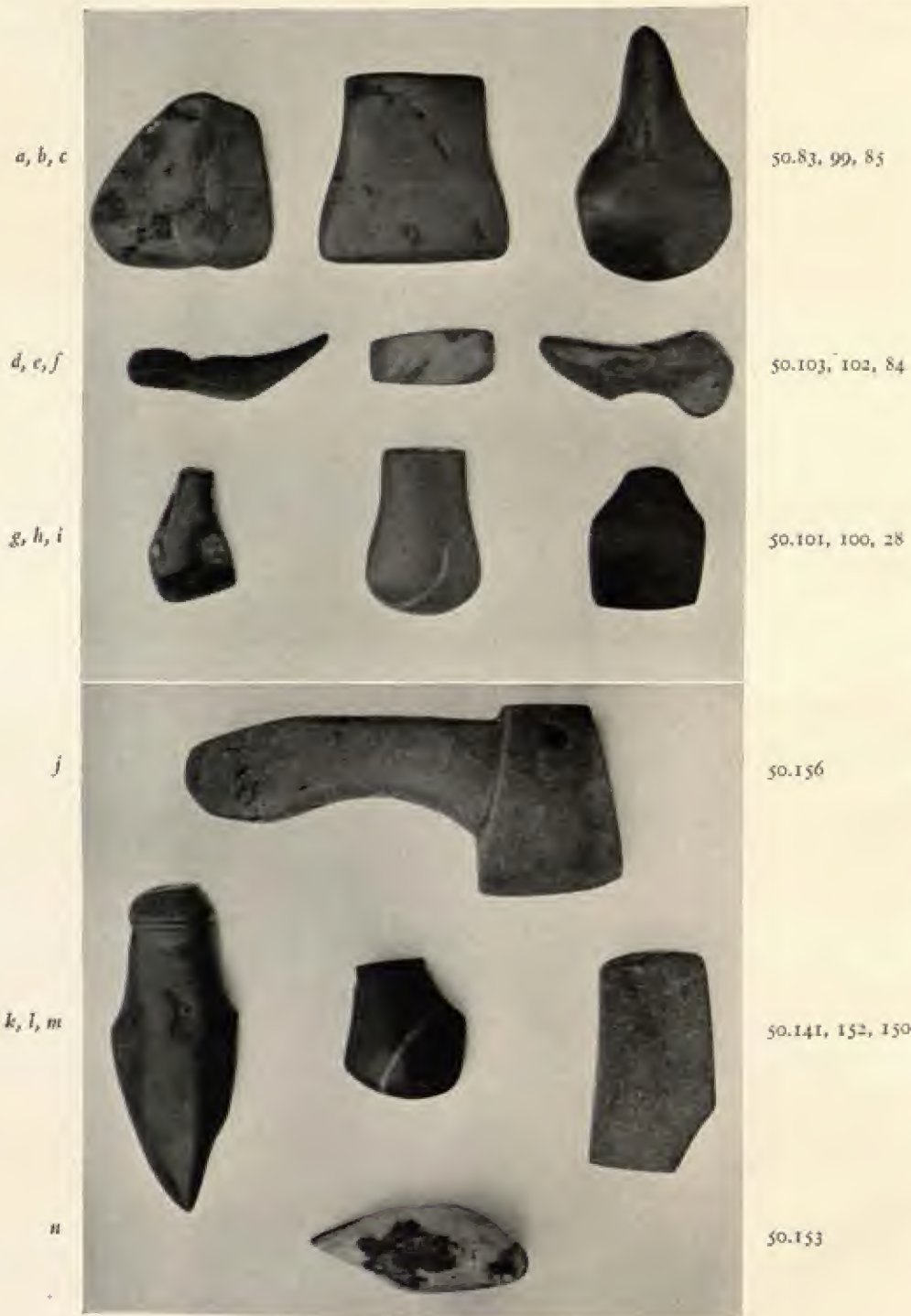
Usk, Monmouthshire

RAGLAN









NEOLITHIC IMPLEMENTS FROM BORNEO

*Photographs: Sarawak Museum*



# NOTES ON SOME NEOLITHIC IMPLEMENTS FROM BORNEO\*

by

TOM HARRISON

*Government Ethnologist and Curator, Sarawak Museum*

**40** In 1947 there were a dozen Sarawak neoliths in the Sarawak Museum, and a few more in other museums. There were also two small series collected from North Borneo by Mr. I. H. N. Evans,<sup>1</sup> one in the Raffles Museum, Singapore, the other in the University Museum of Archaeology and Ethnology, Cambridge. Two more have recently been reported from the Padas River by Mr. H. G. Keith.<sup>2</sup> Two further implements, perhaps mesolithic, from North Borneo, have recently been passed to the Sarawak Museum by the courtesy of Mr. John Mack, Director of the Queensland Museum, Brisbane. A 'club' of Maori type from north-east Borneo, now in the Sarawak Museum, is the subject of a note now with the *Journal of the Polynesian Society*. For the Dutch part of Borneo even fewer specimens and references were available before 1947.

In 1947-49 I was able, while travelling in all three Borneo territories, to collect over 300 more stone implements. These include two types new to the island. One is a conical sago-pounder, perhaps related to pieces from Australia, New Guinea and Java. These are numerous in (but so far confined to) the Murut peoples of north-central Borneo, including the far uplands of the Kelabit plateau.<sup>3</sup> The second type is an adze distinguished by its crescentic cross-section and so far found only in the area south and west of the conical pounders, mainly inhabited by Kayans, Kenyahs and Melanaus. Some of my first specimens were briefly discussed and drawn by Mr. H. D. Collings (*Sarawak Mus. J.*, Vol. V, 1949, pp. 14-22). These two types, to be mapped and fully described at a later date, already disturb the security of some existing generalizations about prehistoric culture successions and movements through Borneo.<sup>4</sup>

During the early part of 1950 I found a third noteworthy sort of neolith, this time a whole group including a wide range of types or varieties, remarkable enough to be put on record immediately (and sought out elsewhere in South-East Asia too).

The first of these implements was brought to me by a Malay in January. It was so handsome and so unlike anything else known to me, that I did not hesitate to pay the \$10 (about 25s.) which he asked for it—I had seldom before paid more than \$1 per neolith. This man was evidently as impressed with the price as I was with the implement. As a result, up to the present date (25 May, 1950), 126 of this group have been obtained, all from Mohammedans along the coastal belt of west Borneo, from Brunei round to the south coast of Dutch Borneo.

It is difficult in a short communication to describe the variety of the series already collected, and there is no indication that this range has been exhausted since new forms come in almost daily. Many clearly belong to a

broad pattern exemplified by a marked vitality and informality linked within one tradition of decisive finish and style. Four sorts of stone are used:

1. Most, including the majority of 'dramatic' forms, are of a soft limestone varying in colour from black (with a high degree of impurity) to dull streaky grey, with crystalline calcite often included, sometimes as a major part of the pattern (see below). This stone has a marbled beauty.

2. Three of the grooved pounders ((iii) below) are of finely grained sandstone.

3. A group made from indurated shale comprises in the main roughly shaped implements, rather different from the rest, but including shouldered adzes and axes near to some in limestone (e.g. Plate Bf). This is closely similar in composition to type 1, but black and dull.

4. A single piece (Plate Bm) is in an igneous rock—probably an andesite porphyry.

Dr. F. H. Fitch and Mr. N. Haile of the British Borneo Geological Survey and Mr. W. Allen of the British Aluminium Company have kindly made these identifications.

Four kinds of implement in this group have some definite relationship to others previously known in the area:

(i) Shouldered adzes and axes, quite similar to some from the north of Malaya (scarce), Burma and Indo-China. These Borneo specimens have a very wide variety of shape and are beautifully finished (fig. 1a, b, c; Plate Bb, c, g).

(ii) A few quadrangular axes and adzes, similar to those found commonly in Malaya, but hitherto scarce in Borneo. Some are very thick and not matched in Malaya (Plate Be).

(iii) Four of the curious grooved implements described by Mr. Evans from Tempasuk in North Borneo (*op. cit.*, figs. 3, 3d), and not previously known elsewhere. None of these new ones show the 'grip marks' which Evans stresses. I suspect they may have been used for pounding. The three I now have in sandstone exactly match Evans's and could be regarded as of a different class from the rest of my collection, if it were not for a fourth made of the same unusual soft limestone as the bulk of the new series. A fine Sarawak adze of roofed type, like some collected by Evans, but in the limestone and calcite, has just such a groove worked into the butt end.

(iv) 'Knives' (some small enough to be suitable for harvesting rice) similar to those from Tembeling in Malaya illustrated by Mr. M. W. F. Tweedie (*J. Polynes. Soc.*, Vol. XLVIII, 1949, fig. 22). Mr. Tweedie examined the Borneo series while visiting Sarawak recently. He considers ours 'much more diverse in shape and much more highly finished.' Some are massive. So far as I know, this sort of implement is only known previously from East Malaya (fig. 1k, and Plate Bd, f).

Thus we have here parallels—in shape more often than in execution and 'mood'—with items from the mainland and from North Borneo, including types only known before from one locality.

But in addition to these there are other forms with no such immediately recognizable relationships. Some idea of the range is given in Plate B and fig. 1. Even so it was difficult to decide which to select out of such wealth of material.

\* With Plate B and a text figure



Fig. 1a and b (museum no. 50.35 and 50.34) are shouldered adzes fairly typical of the group style (*cf.* (i) above).

Fig. 1c (50.75) is an upcurved hafted 'near axe' in soft limestone very close to that in black shale (Plate Bc).

Fig. 1d (50.87) is a fine shouldered axe with two striking features. The shouldering and the butt are rounded and thick, and half the body is shining crystalline calcite, the other half contrasting jet black limestone (another item is a diamond-shaped 'pick,' one side all white calcite, the other black limestone—Plate Bn).

Fig. 1e (50.97) is a pure calcite crystal. The cleavage rhomb

Skinner's New Zealand type IV, 'the most highly specialized of all Polynesian adzes'<sup>5</sup>; the more slender of these were probably used for cutting grooves. There are two other daggers in my series with definitive haft grooves, giving a neck to a defined handle. One, a monster, is 280 mm. long, 65 mm. high and 90 mm. across, the blade tilted forward. These 'daggers' bracket over into the 'Tembeling knives' discussed above (Plate Bk).

Fig. 1g (50.76) is one of several implements shaped rather like butterflies, which could perhaps have been used as planes. Others in the same mood (fig. 1h—50.81) are like vertebræ, one measuring 100 by 110 mm. across.

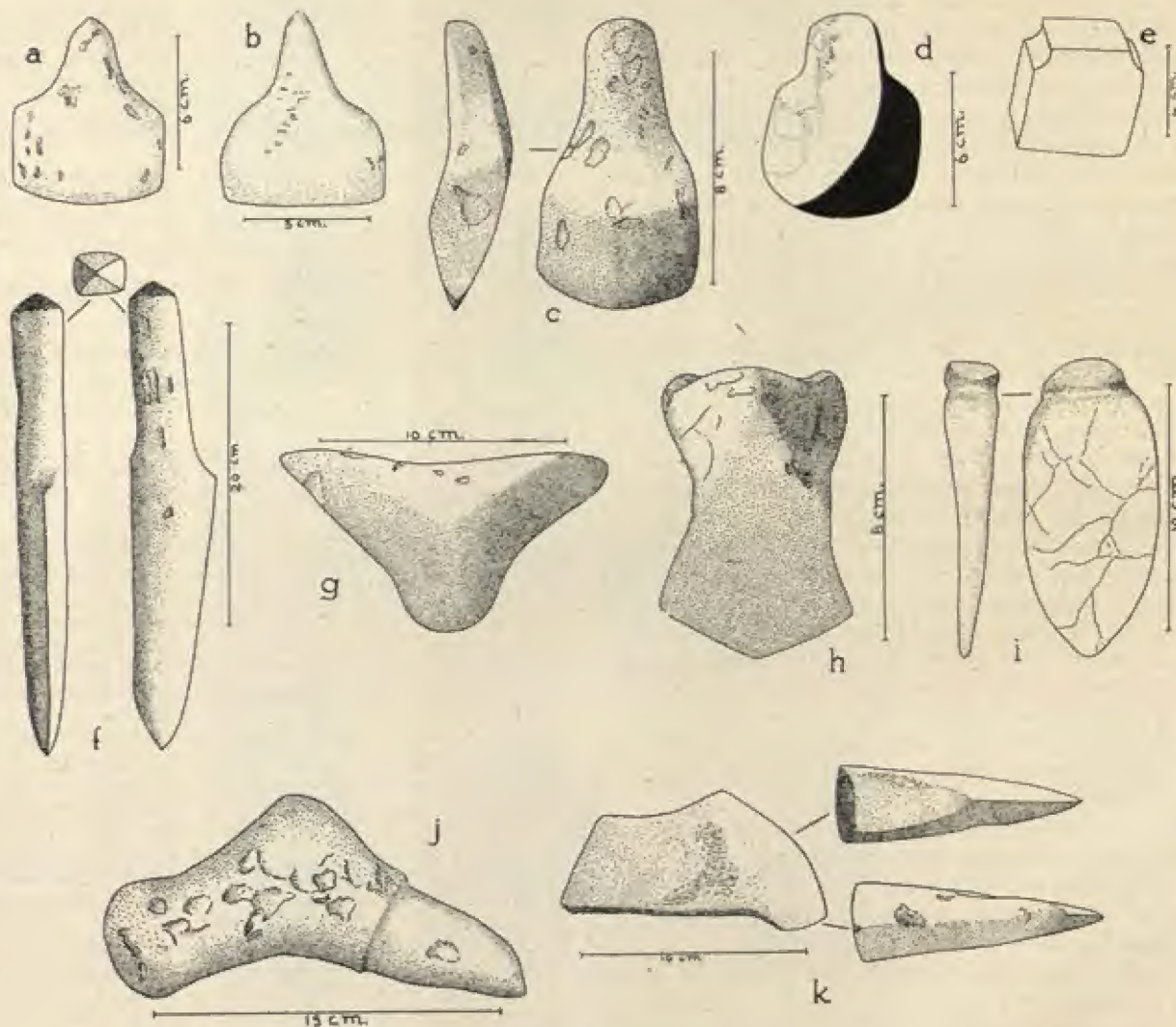


FIG. 1. NEOLITHIC IMPLEMENTS FROM BORNEO

has been shaped along cleavage planes and grooved along two edges to give a shouldered effect. It is like a tilted box in shape, the angles being off the 90 degrees; flat on all surfaces. It is difficult to see what work could be achieved with so soft a stone. One feels that use was only one consideration. Some of these things were surely made with delight, as things of beauty or magic or worship.

Fig. 1f (50.86) is a dramatic sort of 'dagger,' beautifully finished even down to the faceting of the back. This bears a resemblance at least in general 'atmosphere' (it is difficult, at this stage, to be more exact in terminology) to Mr. H. D.

Fig. 1i (50.165) is one of several flat, highly polished stones, usually roughly triangular or heart-shaped in form. Some of these have grooves round a neck or head, possibly to enable them to be suspended. There is no real blade. Relatives seem to be Plate Bk and l.

Fig. 1j (50.79) is one of several massive picks. There are also some small ones, very irregular in shape (*cf.* Plate Bn), one being double-headed, most of them with only one emphatic point and shaped something like a fowl's head. Some have hafted butts, finely finished.

Fig. 1k (50.92) is a 'Tembeling knife,' as in (iv) above.



Other forms include a number of large rough-surfaced shale axes, flat and thin, again varying very much in detail. There are also some pleasant limestone axes somewhere near oval in cross-section and extremely irregular in shape, like Plate Ba and h. One axe is shaped just like a small hatchet, with blade, butt, handle and grip precisely modelled (Plate Bf). Indeed almost every implement is different from the next. A set of these stones together gives the impression of a craftsmanship which revelled in shaping stone and which had an originality as uninhibited as that of Mr. Henry Moore. Conventions and rules, which have shaped the neolith in most eras and areas—and which give to museum cabinets those long successions of drab though instructive similarities—appear to have been barely operative among the makers of these artifacts. The limestone especially lends itself to versatile handling. Rarely found in use elsewhere in South-East Asia, it has the advantage of being easy to work, the disadvantage of losing a cutting edge rather rapidly.

In this part of the world it is usual for neoliths to be regarded as thunderbolts, lightning's teeth, etc., yet very few of the present group were so regarded by the people possessing them. They were usually recognized specifically as ancient implements. All were valued as heirlooms with magical or medicinal purposes; they have often been rubbed or chipped for that reason.

It seems extraordinary that such a wide range of neoliths could have so long escaped outside notice. Nor is it very likely that it is confined to the area which I have so far covered. In particular, the number of shouldered axes or adzes—some of 'advanced' style on the criteria of other students—would seem to complicate the views advanced, for instance, by such a distinguished authority as Professor Heine-Geldern, who states<sup>6</sup> 'the tanged adze is unknown in Indonesia,' and suggests that 'the emigration of the Austronesians from Further India must have taken place prior to the development of the tanged adze,' so that, he argues, this movement introduced the 'Full Neolithic' to

the Archipelago between 2,500 and 1,500 B.C. While I would not suggest that the present Borneo implements are necessarily directly related to those of the Asian mainland and India, we may now have to reconsider any thesis based on their supposed absence from this part of the world (the Celebes shouldered adzes are notably different from the material here discussed, as Professor Heine-Geldern has recognized).

It seems probable that apparently logical sequences of cultural typology have so far been too closely drawn in South-East Asia, and that more study data are required before it is safe—or even wisely suggestive—to erect such sequences upon simple material criteria. The group of 126 specimens here considered in itself raises many question marks. No doubt others remain to be raised. One is, of course, the antiquity of this group. For instance, this material could suggest that in Borneo different 'neolithics' existed side by side, just as today the Kelabits<sup>7</sup> of the interior and the Europeans of the coast live material poles apart within one 'iron age.'

I am grateful to my friend Mr. M. W. F. Tweedie for stimulating suggestions and for the drawings; and to the Sarawak Government for funds with which to obtain the study material. I must stress that these notes are preliminary. It is hoped that over a period of years much fuller information may be obtained.<sup>8</sup>

#### Notes

- <sup>1</sup> MAN, 1913, 86.
- <sup>2</sup> J. Malayan Branch R. Asiat. Soc., Vol. XXIII (1950), p. 148.
- <sup>3</sup> Geog. J., Vol. CXIV (1949), p. 135 and map.
- <sup>4</sup> See my preliminary comment in J. Polynes. Soc., Vol. LVIII (1949), p. 91.
- <sup>5</sup> Proc. IIIrd Congress of Prehistorians, Singapore, 1938, p. 160, figs. 9-12.
- <sup>6</sup> Science and Scientists in the Netherlands Indies, New York (1945), p. 138.
- <sup>7</sup> MAN, 1950, 17.
- <sup>8</sup> Mr. Harrison has promised an appendix to this article, including certain geological evidence, and it is hoped that this will arrive in time for inclusion in the April issue.—ED.

## SOME DIMENSIONS OF THE MILK TEETH OF MAN AND THE LIVING GREAT APES

by

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**41** In commenting on certain observations (Ashton and Zuckerman, 1950) about correspondences between the overall dimensions of the teeth of the Australopithecinae and those of existing apes, Le Gros Clark (1951) has suggested that using the statistical data at our disposal, 'it would be equally possible to state that in no single dimension or index of the deciduous teeth does *Homo* differ markedly from the anthropoid apes.' The present paper records the results of an examination of this suggestion.

#### MATERIAL AND METHODS

##### (i) Dimensions of the Milk Teeth of Existing Apes

48 'overall' dimensions and indices of the milk teeth of the chimpanzee, gorilla and orang-outang have already been published (Ashton and Zuckerman, 1950). A measure of the reliability of the data is provided by the fact that the standard errors of the means of 40 per cent. of the 144 values available were less than 2 per cent. of the mean



itself; of 80 per cent. less than 3 per cent.; of 90 per cent. less than 4 per cent.; and of 97 per cent. less than 5 per cent.

### (ii) *Dimensions of the Deciduous Teeth of Homo*

The dimensions of the milk teeth were measured in skulls belonging to the following collections: 'English,' seventeenth-century Whitechapel series (Cambridge University Museum of Archaeology and Ethnology); 'English,' Spitalfields series of unknown antiquity (C.U.M.A.E.); a series of presumed English teeth (Department of Anatomy, Birmingham University); Anglo-Saxon (British Museum of Natural History); Ancient Egyptian (B.M.N.H.); West African (B.M.N.H.); Peruvian (B.M.N.H.); Tibetan (B.M.N.H.); Eskimo (B.M.N.H. and C.U.M.A.E.); Bushman (B.M.N.H.); Australian (B.M.N.H. and Department of Anatomy, Cambridge University).

In all 331 deciduous teeth were measured. In addition, the dimensions of 11 deciduous teeth of *Pithecanthropus pekinensis* were taken from the descriptions published by Weidenreich (1937). In view of our previous observation that the variability between right and left teeth is insignificant relative to that between different individuals (Ashton and Zuckerman, 1950), the dimensions of corresponding teeth on the two sides of the same specimen were averaged. As a result, our final series of 'teeth' numbered 203.

With the exception of the maximum breadth of the second deciduous molar of modern man, which was not used in the present comparisons, the dimensions investigated were those defined in our previous memoir (Ashton and Zuckerman, 1950). In the case of *Pithecanthropus pekinensis*, we were able to compare only the labial height, maximum labial transverse, and the labio-lingual breadth of the incisors; the labial height, maximum A.-P. dimension, labio-lingual breadth and the index of the canines; the A.-P. length, maximum breadth and index of each molar. The 'maximum indices' of the second molars of the living apes were therefore added to the list which we have already published.

### (iii) *Statistical Comparison*

Each human dental dimension was compared separately with the corresponding averages for chimpanzees, gorillas and orang-outangs, using a *t* test. Means and standard errors were also computed for each human dimension, taking all corresponding teeth together, and these were compared, again by means of the *t* test, with the corresponding averages for apes.

Judging by their standard errors these figures provide as reliable a series of estimates of the overall dimensions of the human milk teeth as those we have determined for the apes. Thus the standard error of the mean was less than 2 per cent. of the mean itself in 60 per cent. of 48 dimensions; and less than 5 per cent. in 98 per cent. Nevertheless, it is appreciated that unbiased means of the dental dimensions of modern 'races' of *Homo* could be derived only from a series of measurements relating to samples of all different 'races,' the size of the samples being proportional to

population size. The material necessary for such a study is not available in England. Our own enquiry has been mainly confined to what are, so far as we know, the two largest collections of human skulls in Britain (the British Museum of Natural History and the Cambridge University Museum of Archaeology and Ethnology).

### RESULTS

In comparing the dimensions of the human and ape teeth, differences have been regarded as significant when they gave values of  $P < 0.02$ . At this level of significance we should, on an average, expect that not less than one ape in fifty would differ significantly from the mean of its own species.

Out of the total of 203 human teeth compared, 42 differed significantly in one or more dimensions and indices from the orang-outang, 66 from the chimpanzee, and 170 from the gorilla. On the other hand, in the whole series of 203 teeth there were only 8 which differed significantly from all three apes.

The differences between the dimensions and indices of the human and ape milk teeth became more apparent when the means were compared (138 comparisons, comprising 46 dimensions and indices for each type). Most of the human dimensions proved to be significantly smaller, and the indices significantly different from those of the apes (Table 1).

### DISCUSSION

This analysis shows that the overall dimensions, and related indices, of the human milk teeth are significantly different from those of the three great apes. It therefore does not bear out Le Gros Clark's suggestion that the contrary may be true. A similar result emerged from a comparison of the overall dimensions and related indices of the permanent teeth.

It should be emphasized that in both cases the differences became more apparent in the comparison of means than when the dimensions of individual human teeth were compared with the means of the ape teeth. It is readily understandable why the differences between the dimensions of the human and ape teeth show up better in the comparison of means, for the precision of an estimate varies with the size of the sample from which it is derived, and the information provided by a single specimen is extremely limited compared with that yielded by a number.

Our previous study of the overall dimensions of twelve Australopithecine milk teeth (Ashton and Zuckerman, 1950) showed that only one differed significantly from the corresponding tooth of each great ape. Le Gros Clark (1951) has pointed out that the overall dimensions of one of these teeth (a lower canine) was incorrectly given in the memoir from which we quoted (Broom and Schepers, 1946), and has now provided revised figures. These, too, are not significantly different (at a level of  $P < 0.02$ ) from the corresponding values for the great apes. In the interval since our earlier study was submitted for publication the overall dimensions of 10 more Australopithecine deciduous



teeth have been published (Broom, 1950; Broom, Robinson and Schepers, 1950), and there are now available for comparison 22 teeth in all (1 upper canine; 2 upper first molars; 2 upper second molars; 1 lower central incisor;

TABLE I.—COMPARISONS BETWEEN THE MEAN DIMENSIONS OF THE MILK TEETH OF MAN AND THE APES

Tooth	Dimension	Comparisons between man and chimpanzee		Comparisons between man and gorilla		Comparisons between man and orang-outang	
		P	±	P	±	P	±
Upper first incisor	Max. lab. trans.	<0.001	—	<0.001	—	<0.001	—
	Lab. height	0.1-0.05	—	0.4-0.3	—	<0.001	—
	Max. lin. trans.	<0.001	—	<0.001	—	<0.001	—
	Lin. height	0.02-0.01	—	0.5-0.4	—	0.02-0.01	—
Upper second incisor	Lab.-lin. breadth	0.05-0.02	—	0.01-0.001	—	<0.001	—
	Max. lab. trans.	<0.001	—	0.01-0.001	—	<0.001	—
	Lab. height	0.02-0.01	+	0.01-0.001	+	0.9-0.8	+
	Max. lin. trans.	0.01-0.001	—	0.05-0.02	—	<0.001	—
Lower first incisor	Lin. height	0.5-0.4	+	0.05-0.02	+	0.2-0.1	—
	Lab.-lin. breadth	0.7-0.6	—	0.7-0.6	—	<0.001	—
	Max. lab. trans.	<0.001	—	0.2-0.1	—	<0.001	—
	Lab. height	<0.001	—	0.4-0.3	—	0.01-0.001	—
Lower second incisor	Max. lin. trans.	0.01-0.001	—	0.3-0.2	—	<0.001	—
	Lin. height	<0.001	—	0.2-0.1	—	<0.001	—
	Lab.-lin. breadth	<0.001	—	0.01-0.001	—	<0.001	—
	Max. lab. trans.	<0.001	—	0.1-0.05	—	<0.001	—
Upper canine	Lab. height	0.8-0.7	—	0.05-0.02	—	<0.001	—
	Max. lin. trans.	<0.001	—	0.05-0.02	—	<0.001	—
	Lin. height	<0.001	—	0.01-0.001	—	<0.001	—
	Lab.-lin. breadth	<0.001	—	<0.001	—	<0.001	—
Lower canine	Index	<0.001	+	<0.001	+	<0.001	+
	Lab. height	<0.001	—	<0.001	—	<0.001	—
	Lin. height	<0.001	—	<0.001	—	<0.001	—
	Max. A.-P. dim.	<0.001	—	<0.001	—	<0.001	—
Upper first molar	Lab.-lin. breadth	0.05-0.02	+	<0.001	—	<0.001	—
	Index	<0.001	+	<0.001	+	<0.001	+
	A.-P. length	0.3-0.2	—	<0.001	—	<0.001	—
	Max. breadth	<0.001	+	<0.001	—	<0.001	—
Upper second molar	Index	<0.001	+	<0.001	+	0.2-0.1	+
	A.-P. length	<0.001	+	<0.001	—	<0.001	—
	Trigone breadth	<0.001	+	<0.001	—	0.01-0.001	—
	Talon breadth	<0.001	+	<0.001	—	<0.001	—
Lower first molar	Trigone index	0.9-0.8	—	<0.001	+	0.1-0.05	+
	Talon index	0.8-0.7	—	<0.001	+	0.05-0.02	+
	A.-P. length	0.7-0.6	—	<0.001	—	<0.001	—
	Max. breadth	<0.001	+	<0.001	—	0.01-0.001	—
Lower second molar	Index	<0.001	+	<0.001	+	<0.001	+
	A.-P. length	<0.001	+	<0.001	—	<0.001	—
	Trigone breadth	<0.001	+	<0.001	—	0.1-0.05	—
	Talon breadth	<0.001	+	<0.001	—	0.05-0.02	—
Lower first incisor	Trigone index	<0.001	+	<0.001	+	<0.001	+
	Talon index	0.01-0.001	+	<0.001	+	<0.001	+

± indicates that the mean dimension for man is larger than that of the ape.

Abbreviations: Max.=maximum; lab.=labial; trans.=transverse; lin.=lingual; A.-P.=anterior-posterior

The results are summarized in Table II, from which it will be seen that only one of the 22 Australopithecine teeth differed from all three great apes, whereas seven differed from man. On the face of it, the Australopithecine milk teeth correspond in their overall proportions more to the ape than to man.

In view of the results of the comparisons of human and ape milk teeth, it might be asked whether significant differences between the overall dimensions of the teeth of the

TABLE II.—COMPARISON OF 22 DECIDUOUS TEETH OF AUSTRALOPITHECINE WITH THOSE OF MAN AND THE GREAT APES

No. of teeth examined	Chimpanzee	Numbers not differing in any overall dimension or index from			Man
		Gorilla	Orang	Chimpanzee, orang and gorilla	
22	7	12	19	21	15

fossils and existing apes might not emerge if it were possible to compare their corresponding means. That they might well do. But as our estimates of the basic statistics of the human and ape teeth are equally precise, the fact that our comparisons of the dimensions of the fossil teeth showed more similarities with the ape than with the human deciduous dentition implies that differences would be expected to emerge less frequently in a comparison with the means of the ape than in one with those of the human milk teeth. Unfortunately, the Australopithecine milk teeth whose dimensions are published are not only few in number but also attributed to different species and genera. Consequently, the best information about resemblances and differences that can be obtained about overall dimensions must for the moment be derived, as we have done, from a comparison of individual dimensions of the fossils with the corresponding means for apes and man. If, however, the question of species attribution is set aside, we can compute means and estimates of error for some overall dimensions of the lower first milk molar, and lower second molar, the data for the other teeth being too few, or in the case of the lower canine, too heterogeneous. Judging by the means and standard errors of the maximum length, the maximum breadth, and the index derived from them, the molars form a fairly homogeneous series so far as overall size and shape are concerned. A comparison of the means for the three overall dimensions and indices shows that the second molars correspond with one of the apes (the orang), but not with man, and that the first molar corresponds with neither the apes nor man.

Our best thanks are due to Dr. F. C. Fraser (Department of Osteology, British Museum of Natural History), Mr. J. C. Trevor (Cambridge University Museum of Archaeology and Ethnology) and Professor H. A. Harris (Department of Anatomy, Cambridge University), for allowing access to the skulls in their charge. The expenses incurred in the computing were met by a grant from the Royal Society; those incurred in the fieldwork were met by a grant from the Viking Fund.

2 lower lateral incisors; 4 lower canines; 4 lower first molars; 6 lower second molars). We have now compared all these, dimension by dimension, and index by index, with the corresponding teeth of the great apes and of man.



## SUMMARY

1. The overall dimensions and indices of the milk teeth of man have been compared with those of the great apes. Most of the human dimensions are significantly smaller and the indices significantly different, at a level of significance  $P < 0.02$ .

2. The overall proportions of 22 Australopithecine deciduous teeth appear to resemble those of the great apes more than they do those of man.

## References

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## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

### The Journal of the Royal Anthropological Institute

**42** The Council has decided that for the present, in order to accelerate the clearing-off of the current arrear of publication, both parts of each volume of the *Journal* shall be published bound together, as was done for a time during the war. Volume LXXVIII, Parts 1 and 2, will be published shortly and will contain the following papers:

'The Integration of Anthropological Studies' (Presidential Address), by Professor Daryll Forde.

'Some Aspects of Political Organization among the American Aborigines' (Huxley Memorial Lecture), by Professor R. H. Lowie.

'Religious Belief and Personal Adjustment' (Henry Myers Lecture), by Professor Raymond Firth.

'Ritual and Secular Uses of Vibrating Membranes as Voice-Disguisers,' by the late Professor Henry Balfour, F.R.S.

'Celtic Origins: A Stage in the Enquiry,' by T. G. E. Powell.

'Jando, Part II,' by H. Cory.

Minutes of the Annual General Meeting, Reports of the Council and of the Honorary Treasurer, and combined index of the *Journal* and *MAN* for the year 1948.

Work is also well advanced on Volume LXXIX, Parts 1 and 2, which will contain the following papers:

'Integrative Aspects of the Yakō New Season Ritual in Liboku' (Presidential Address), by Professor Daryll Forde.

'Nyakyusa Age Villages,' by Professor Monica Wilson.

'The Perception of Time and Space in a Situation of Culture Contact,' by S. N. Eisenstadt.

'Canoes and Navigation of the Maya and their Neighbours,' by J. Eric S. Thompson.

'The Physical Type of Certain Peoples of South-East Nigeria,' by G. I. Jones and B. Mulhall.

'Measures of Divorce Frequency in Simple Societies,' by J. A. Barnes.

'Iku-nishi of the Saghalien Ainu', by Dr. Moses Osamu Baba—An archaeological paper.  
 Minutes, etc., as for Volume LXXVIII.

### The Library of the Royal Anthropological Institute

**43**

#### Request for Offprints

The Institute Library holds long runs of most of the anthropological journals published throughout the world, but where articles of anthropological interest are published in journals not dealing specifically with anthropology the Institute has to rely upon gifts of offprint copies from the authors concerned. It would greatly assist the Librarian if Fellows, and indeed all anthropologists, could arrange to send to the Institute an offprint of any article they publish, especially where the periodical concerned is not one of those regularly taken by the Library; a list of these periodicals, dealing largely with anthropological subjects, was published in the Institute's *Journal*, Vol. LXXVI, part 2, and additions are noted from time to time on the cover of *MAN*.

#### Request for Book Suggestions

Considerable progress has now been made with the reorganization and reclassification of the Institute's Library. As expected numerous important gaps in the Institute's collection are coming to light and every effort is being made to make good these deficiencies. Gaps and deficiencies in periodical series resulting from the war and from loss have in nearly all cases been made good. Deficiencies in the book collection are less easily noticed, and users of the Library are asked to notify the Librarian whenever they find that the collection lacks some volume of anthropological importance.

## SHORTER NOTES

'*MAN*' in the Eighteenth Century. By Sir John L. Myres, O.B.E., F.B.A., first Honorary Editor of *MAN*

**44** On Wednesday, 1 January, 1755, appeared the first number of *MAN*, A PAPER for ennobling the SPECIES: Designed to be continued weekly. It was published by subscription, and my copy, bought in Paris, has the name of 'Mrs. Ann Whitwell' printed on a flyleaf. The printer was John Haberkorn, London, and it was to be sold by J. Robinson at the Golden Lion in Ludgate Street and A. Linde in Catherine Street in the Strand [Price Two Pence]. Linde's name disappears after No. 5, when the printer was in Gerrard Street. Robinson also fades out after No. 44, and Haberkorn offers 'whole Sets or any single Number,'

till No. 53 on 31 December, 1755, which is 'no longer designed to be continued weekly' and ends: 'N.B. For the future it is thought more advisable to continue the design of these papers in pocket volumes, occasionally, than in single sheets.' I have no record of such 'pocket volumes.'

The 'single sheets' (11 inches × 6½ inches) contain six pages, or occasionally eight, but sometimes p. 6 is blank. There is a woodcut heading, uniform, showing a Muse, with a globe, book, lyre, palette, and other emblems, and a tailpiece, not quite uniform, where there is room for it. The printing is careful, with uniform woodcut initial in each number. My copy is half-bound in mill-board with marbled paper backs and calf corners. The spine has



been reinforced with brown paper, to which the original label, MAN, on tooled and gilded calf, has been affixed.

There are no signs of authorship, nor any allusions to individuals throughout, except a couple of loyal references to His Majesty [George II]. The signatures of occasional 'correspondents' are fictitious. The programme is ambitious:

'The authors of the paper, desiring to have numerous readers, will endeavour to be instructive, and entertaining. This end cannot be obtained without making choice of a proper subject. And upon examining all the sciences, we can find no subject more instructive, or entertaining, than man. . . . Men in general have a love for themselves. . . . We may therefore hope to gratify our readers by a weekly paper containing news of themselves. . . . Men carry their researches through all nature . . . and yet remain ignorant of themselves, the causes whereof will be assigned in the course of our papers . . . discussing religion, morality, and taste. . . . We write for the improvement of men, and will first endeavour to humanize ourselves. . . . In due time, proper mention shall be made of the authors.'

—a promise never fulfilled.



The subsequent numbers are in the same strain; but there are some more specific essays. No. 4 announces a letter signed 'Woman' which is printed in No. 5, pleading for more 'manly' treatment of her sex. No. 6, on 'laughing,' a peculiarly human trait, acknowledges an unsigned letter on 'taste,' which is not printed; in No. 14 is one signed 'Prettyman' on the good looks of men, women, and brutes. This draws a letter from 'Urania' (No. 17) asking matrimonial advice, and in No. 18 the character is sketched of 'Miss Wasp' a spoiled child: her matrimonial adventures are forecast but never printed. In No. 20 a 'Christian Gentleman' condemns 'scoffers.' In general *Man's* philosophy is Christian, with occasional reproof of 'deists' and 'free-thinkers'; no other religion is even mentioned.

In No. 13 a ne'er-do-well, signed 'VICE CARTER', propounds a project for collecting material for comment; in No. 21, he appears with his cart, several queer characters, and a trunk full of

specimens; quack medicines, rejected manuscript, poems, and a set of bishop's robes, from a conceited curate never likely to need them. 'Carter' has already had his portrait painted. He reappears in No. 33 after a road accident, with a dram-drinking widow who has given him much trouble. This mild fun soon palls, and the best of the later essays are on education and the management of children (Nos. 45, 46, 48). After a meditation on death (No. 52) the character is drawn of a 'noble virtuous man, a true picture of real life.' When his valet went to bed drunk in his dressing room, he had him made comfortable 'in rich night gown' and effected a perfect cure. But by this time the fate of *Man* was settled, and the 'ennobling of the Species' had to wait till 1901.

#### Note

Sir John Myres has generously presented his copy of this periodical to the Royal Anthropological Institute, where it may be consulted by the curious; No. 53 has with his permission been extracted to complete the British Museum copy, and replaced by a photostat.

It was hardly to be expected that our predecessors of 196 years ago should have a perfectly clear premonition of the scope and nature of what was to be the science of anthropology. Yet it is not too fanciful to see in *Man: A Paper for Ennobling the Species* a notable manifestation of one of the currents of humane thought which were to merge, in the following century, after the success of the anti-slavery movement, in the new science and its chief centre, the Ethnological Society of London. That the authors of the periodical would have approved of that society and its successors—and also of such particular instruments of anthropological advance as the Emslie Horniman Anthropological Scholarship Fund and the Colonial Research Fellowships—is made clear by the closing paragraph of the 32nd issue, dated 6 August, 1755:

The erecting of academies, and societies, for promoting natural knowledge, is the glory of modern Europe; and whoever wishes well to mankind, must needs wish well to such societies; whose design it is to promote discoveries for ennobling the species. And it might highly contribute to this glorious end, if funds were raised for encouraging inventors; and for promoting the business of experimenting at large, in such societies, under judicious regulation, and a conduct nobly frugal.

There seems no reason why *MAN: A Monthly Record of Anthropological Science* should not similarly 'endeavour to be instructive, and entertaining,' especially as it certainly desires 'to have numerous readers.' Therefore let our contributors not too rigorously eschew the urbane wit and humour which distinguish them in conversation and might judiciously relieve the proper gravity of their writings—provided that their length be not added to thereby.

The mention of humour as an instrument of literary style in anthropology may, furthermore, lead us to inquire why humour, like the arts, has, especially of late, received such scant attention as a subject of study among primitive and other peoples; for no one will deny that it has an important function in society. The great Sir Richard Burton (for whom ethnology was certainly a humanity) compiled *Negro Wit and Wisdom* in 1864 (not to mention the Benares edition of *The Thousand Nights and a Night*); but he has had few followers, and for no good reason humour is not yet considered entirely respectable.—ED.

#### The Institut Fouad 1<sup>er</sup> du Désert, Heliopolis, Egypt

**45** The establishment is announced of the *Institut Fouad 1<sup>er</sup> du Désert* at Heliopolis, near Cairo. Its objects are to study deserts from all aspects, geological, botanical, zoological, anthropological, etc., and to encourage such studies; to investigate the opportunities they present for agricultural or industrial development, and the means of preventing their extension into fertile lands. The institute's site provides space for a botanical garden, a museum, and a library. A comprehensive index of relevant references will be compiled, and it is intended to produce a periodical review.



## REVIEWS

## ARCHAEOLOGY

**Metallurgy in Antiquity.** By R. J. Forbes. Leiden (E. J. Brill), 1950. Pp. 489. Price 19 guilders.

46

The appearance of a large book on metallurgy in antiquity by the well-known authority Professor R. J. Forbes is of much importance to archaeologists and to those metallurgists who are interested in the early history of metal. The work covers the origin and early history of the ferrous and non-ferrous metals used in antiquity, with a full discussion of the archaeological background. In a work covering so large a time scale, the matter of archaeological chronology is always a matter of considerable difficulty, and in reading the book it must be remembered that, though the work is published in 1950, printing was completed during the Second World War and therefore the author has been unable to incorporate the latest chronological evidence; as he points out, the estimate of the prehistoric periods will have to be considerably shortened, and used with care so that their relative value may not be misunderstood. In dealing with the archaeological aspect of the various problems the author gives a thorough discussion with adequate bibliographical references at the end of each chapter. I do not feel competent to venture an opinion on the extremely complex subject of the chronological relationships and cultural movements involved in the origin and development of early metallurgy in Europe and the Near East. It is, however, particularly satisfactory to note that Professor Forbes refuses to follow the late Dr. Witter's unfortunate and misguided attempts to establish an independent origin, which would owe nothing to the vastly older Near Eastern complex, for Central European metallurgy. In connexion with the probable sources of the metals, a series of admirably clear distribution maps showing the major deposits of the various metallic ores will be of considerable value to the student.

The technological aspect of the metals in antiquity is treated in a very clear and comprehensive manner by Professor Forbes. Much of the technical information presented is new, or hitherto only available in technical literature unfamiliar to the archaeologist. The place of native copper in the Old World is not treated at length, but the discussion given of the varieties of native copper, and of the various methods of working it, is welcome. Professor Forbes was not, of course, in possession of the recent reports published by the Ancient Mining and Metallurgy Committee of the Royal Anthropological Institute, which has carried out some preliminary work on this subject. However, he very properly stresses the need for further analyses to enable what may be termed the "annealed native copper phase" to be reviewed. Tables of analyses are not given in the book, and it would have been helpful to British research workers had the author given a table of references to where the more important analytical work of continental scientists has been published. A long and interesting chapter is devoted to the origin and development of copper-working from the earliest times. The author expresses early copper metallurgy in five major stages with a separate stage allocated to the smelting of sulphide ores. His comprehensive treatment of the sulphide ores is particularly appropriate in view of the confusion in past literature concerning the use of the sulphides. If sufficient archaeological evidence is forthcoming it may be found necessary to introduce another stage, or sub-stage, to cover the melting and casting of native copper prior to the author's Stage C. Concerning the transition to bronze, the known sources of tin are well reviewed. If it ever becomes possible to analyse a due proportion of the Irish celts, the statement (p. 242) that the tin deposits of Co. Wicklow cannot be considered as a source of primary importance may have to be modified. Opinion as to the value of the Wicklow tin is divided. Coffey considered that there was a considerable quantity of tin in the Avoca valley, while Armstrong did not view the deposits as of much extent. In the chapter devoted to iron, the various theories to account for the origin and development

of the metal are given. Here, the author makes the important point that the technical history of the early iron industry will remain obscure until we know more about the evolution of the various furnace types; indeed, we may say that the two things are so inter-related that, for further knowledge of the history of primitive iron-working, the most pressing need at the moment is for more information concerning the evolution of the furnace. On the subject of cast iron, Professor Forbes rightly mentions (p. 407), that cast iron seems to have been known to the Graeco-Roman world as an accidental and useless product. Evidence would appear to be accumulating to show that cast iron was occasionally produced as a product of overheated iron furnaces, even during the prehistoric periods.

There are a number of small slips through the text, such as lazurite for azurite (p. 30), and 'native gold' (p. 326) should read 'native copper.' Also (p. 70) the experiments which I carried out were conducted not at the Royal School of Mines, but with facilities provided by the Borough Museum, Newbury. However, these are but small matters, and there is no question that Professor Forbes's work is an exceedingly important and valuable contribution to the study of ancient metallurgy. H. H. COGHLAN

**Zinc and Spelter: Notes on the Early History of Zinc.** By J. M. Dawkins. Oxford (Zinc Development Assn.), 1950.

47

Pp. 35. Distributed free

The problem of zinc in antiquity has not received a prominent place in recent technological literature and there was a decided need for a work dealing in a clear manner with its discovery and history. The author and the Zinc Development Association are therefore to be congratulated on the production of this little book which will be welcomed by archaeologists and others who are interested in the early history of the metal.

The addition of a certain proportion of zinc to copper gives what is now known as brass, and the author does service in so clearly dealing with the use of calamine ore, and in stressing that the production of metallic zinc alone is a decidedly complex process, almost certainly unknown before the Middle Ages. The view that the discovery of metals other than native (p. 6) was made in domestic fires towards the end of the later Stone Age (say 6000 B.C.) would, in the light of present knowledge, appear to place the discovery of smelting at an unduly early period. Also, any evidence which points to the domestic fire as the original smelting furnace would appear to rest on very slender foundations.

Considerable space is devoted to the work of the alchemists, with useful European references to zinc from the sixteenth century onwards. In view of its long history it may appear strange that zinc was not known and smelted as a separate metal until so late. Here, the evidence put forward is of considerable value. The space devoted to spelter and pewter is, unfortunately, very short, but it should be useful in removing any possibility of confusion between the lead-tin alloy which we properly term pewter, and the copper-zinc alloy usually known as spelter, which is used commercially in brazing operations.

The book ends with five appendices giving selected references from ancient sources. H. H. COGHLAN

## Note

It may be worth pointing out, as a pendant to this urbane and scholarly work, that the famous 'bronzes' of Ife (see, e.g., MAN, 1949, 1) are in fact zinc and lead bronzes, containing in some cases a surprisingly large amount of zinc; and that, when further work has been done on the analysis of West African castings, this may perhaps prove to be a determinant of pre-Portuguese manufacture.—ED.



## AMERICA

**The American Indian: An Introduction to the Anthropology of the New World.** By Clark Wissler. New York (Peter Smith), 1950. Pp. xvii, 466, 82 illus. Price \$5.50

**48** This is a new printing of the third edition (1937) of this well-known book. Clark Wissler will always be remembered for his invigorating attempts to produce a synthesis of the anthropological and archaeological problems of the Americas, and this book has had a wide and deep influence on students since its first appearance in 1917. It is good, therefore, to know that the book is still obtainable. Clark Wissler would undoubtedly have used this opportunity to incorporate in his book developments since 1937, and it is to be hoped that the time will soon come when a new comprehensive statement on the problems of the Americas will continue his pioneer work.

S. J. JONES

**Decorative Designs of the Ojibwa of Northern Minnesota.**

By Sister Bernard Coleman, O.S.B. Washington (Catholic Univ. of America), 1947. Pp. xvi, 125, 17 plates, 32 text figs.

**49** This dissertation deals with typical Ojibwa designs of two periods, before and after the impact of white culture, and describes them against their historical background.

Most information and material was available for the period 1870-1920 and the influence of Western civilization is clearly shown in the treatment and predominance of floral motives, particularly in the bead work, whilst the geometric patterns were suggestive of Sioux or other tribal intrusions. The designs of the earlier period, 1830-1870, tended to be geometric and zoic, and were traditional, though none can be traced back more than 100 years. Floral patterns of this period were simple conventional forms of indigenous plants, gathered by the women and copied. Material, social and religious cultures all had typical patterns; the floral and geometric ones used for utilitarian purposes had no symbolism, but those used socially included animals, often as tribal or personal emblems. These eponyms marked trails, indicated ownership, conveyed messages, and, upside-down, marked graves. Designs associated with religious culture concerned manitos, dreams, charms for love and curing sickness, as well as those used by the Midéwiwin (Grand Medicine Society) in their ceremonies. Birch-bark scrolls were the means by which ceremonial tradition was handed down and an interesting illustration of one is given. On it are scratched crude pictographs representing the four degrees of the society, and, amongst other figures, the ancestral circle of the Ojibwa, bear, otter, fish, thunder-bird, serpent, men and spirit forms occur. All the designs lack detail. These and other ceremonies are described in the text.

In olden times symbolic patterns were important factors in inculcating social and religious concepts. Lacking the high moral standard of the Midé teaching the young Indians have now lost interest in preserving the old designs and commercialism has largely destroyed good standards of craftsmanship, so Ojibwa art is fast disappearing and this book will be a useful record of past achievement.

LAURA E. START

**The Origins and History of the Mayas, Part I.** By M. Wells Jakeman. Los Angeles (Research Publ. Co.), 1945. Pp. 203.

**50** 2 maps, 2 diagrams, 10 plates and 2 text figs.

The author is of opinion that a new approach is called for in order to advance the study of the Maya civilization and its place in history and he aims to do this in a work in three parts of which this is the first. In it he outlines the present state of knowledge of the subject. Part II will contain an account of a discovery he claims to have made of a new chronological count, and of other new conclusions respecting *inter alia* the origins and antiquity of the calendar, and Part III will contain final narrative reconstructions.

The present outline is in general accurate as far as the calendar and writing are concerned, except that Dr. Jakeman inclines to favour the late B. L. Whorf's views on the nature of the writing. I do not think that there is any real evidence in favour of Whorf's theories, but this is not the place in which to discuss them. The attempted translation on Whorfian principles of a text from the Codex Tro-Cortesianus is, in my opinion, of no value.

One is glad to see that he discards the word *tzolkein*; the name Sacred Almanac, or Almanac, which he, following Thompson, employs, is much the best term. One could wish he had also discarded the word *haab*, and had substituted 'vague year.' Apparently he has not seen my paper 'Some Maya Time Periods' in the *Proceedings of the XXI Congress of Americanists* (Gothenburg, 1924), in which I show that *haab* was used only for the period which is also called *tun*, i.e. the period of 360 days, and that no name was used for the 365-day year. Both Mr. Thompson and the late Dr. J. E. Teeple have adopted this view. 'Vague year' is the usual word used for the ancient Egyptian year of 365 days, and it is desirable to use it for the same period in Maya chronology. Another paper of mine which the author seems not to have seen is 'The Highest Known Maya Number' (MAN, 1919, 20), for he cites the late Dr. Herman Beyer's paper of 1936, 'The True Zero Date of the Maya.' Beyer had a copy of my paper but did not in his article acknowledge that the idea of this Zero date was originally due to me. In the same paper Beyer announced the value of the *pictun* sign in the Temple of Inscriptions. This discovery also was first made by me (MAN, 1923, 39), and Beyer had a copy of this paper too, but did not acknowledge it in his own article.

On lesser matters, I would not say (though it frequently has been said) that the Maya calendar was a count of days. The Books of Chilán Balam always reckon in *haab* (i.e., 360-day periods). But of course, from the beautiful symmetry of the Maya system, this period always contained the same number of days, so it is a question mainly of terminology. The American currency might similarly be described as a count of cents, not dollars.

There seems to be a slip in the statement of the period endings. A full period ending (if this means a date fixed in the cycle, *baktun*) would not return till after 374,400 vague years, not 18,720 as stated, and the other periods should be correspondingly revised. See Bowditch, 1910, p. 323.

One is glad to see that the author strongly favours the Goodman-Martinez-Thompson correlation.

He gives a revised translation of part of the Chilán Balam of Mani, and publishes for the first time the text and an English translation of the Historical Recollections of Gaspar Antonio Chi.

RICHARD C. E. LONG

**Cochise and Mogollon Sites, Pine Lawn Valley, Western New Mexico.** By Paul S. Martin, John B. Rinaldo and Ernst Antevs. *Fiediana: Anthropology*, Vol. XXXVIII, No. 1, Chicago Nat. Hist. Mus., 29 April, 1949

**51** Some years ago, when it was first proposed to set up Mogollon as a culture separate from Anasazi and Hohokam, there was considerable opposition to the idea; to a number of specialists in South-western archaeology, the variation from Anasazi and Hohokam in Mogollon pottery types, artifact forms, burial techniques, and pit-house constructions did not seem sufficiently distinctive to justify regarding Mogollon as a separate culture. As the information has increased, however, the opposition has gradually died; and there now remain but a few who do not accept the existence of this third group.

This fuller information has come from the work of the archaeological expeditions of the Chicago Natural History Museum to the Pine Lawn Valley, near Reserve, New Mexico. Paul S. Martin and John B. Rinaldo have done an excellent job in collecting and assembling these data, some of which appear in *Cochise and Mogollon Sites*, their report of work done in the summer of 1947.

One of the pressing problems of the Mogollon culture is to discover its source; and in this report the theory is advanced that the Mimbres branch of Mogollon developed out of the Cochise culture. Ernst Antevs determined geological dates for certain strata containing Cochise artifacts, thus helping to complete the sequence of events in this area. Since erosion cycles play a very important part in the method of dating, this geological technique is discussed at length with special reference to the climatology of the region.

The rest of the report deals mostly with the discoveries made in the excavation of sites of three phases of Mogollon, the Pine Lawn,



Three Circle and Reserve. The description of these phases includes a very thorough discussion of pottery types found that season and an excellent chart showing pottery frequencies in all the phases at Mogollon sites dug by them to that date.

The report contains admirable illustrations, charts, and plans. The artifact forms are photographed and described, as are the pottery types. Aside from a general description, each pit house is shown in a photograph, plan, and cross-section. In addition to the previously mentioned pottery tables, there are several charts showing climatic variations and an excellent diagram illustrating the development of traits of the Mimbres branch of Mogollon.

DONALD E. THOMPSON

**A Naturalist in the Gran Chaco.** By Sir John Graham Kerr. C.U.P., 1950. Pp. xi, 235, 23 plates, 2 maps. Price 21s.

**52** The eminent zoologist who is the author of this book was in 1889-91 attached as naturalist to an expedition exploring the Pilcomayo, a river which for most of its length is the boundary between the Argentine and Paraguay. He made friends with a band of Indians belonging to the Natokoi branch of the Tobas, and lived with them for some time. He gives a very interesting account of their hunting, travelling and camping. Though it is often very cold at night they had no huts or tents, but in case of heavy rain might tie a mat to a tree or bend some branches together to make a rude shelter. Having no hard stone in their country, they made nearly all their weapons and tools of wood, a few iron ones replacing those of stone formerly obtained from the distant Andes.

They had no words for numbers, merely holding up fingers, never beyond seven, when it was necessary to express them. They recognized the phases of the moon, and knew the seasons by their fruits, but had no idea how many days there were in a month or months in a year. In spite of this the author describes them as being of high intelligence as well as fine physique.

His account of the Natokoi occupies only about one-sixth of the book, of which the rest is a very readable account of the local fauna and of the author's adventures in obtaining specimens. There are about 40 illustrations, mostly from the author's photographs.

RAGLAN

**The Maya Calendar of the Ixil of Guatemala.** By J. Steward Lincoln. Carnegie Institution of Washington, Publ. 528, Contrib. to Amer. Anthropol. and Hist. No. 38. Washington, D.C., 1942. Pp. 31 and a map.

**53** The author of this paper unfortunately died while in the field just after he had completed the manuscript. It has been published with very useful notes by J. Eric S. Thompson and is one of the most important studies of the functioning of the ancient calendar among modern Indians which has ever been published, ranking with the work of La Farge and Byers among the Jacalteca and of La Farge at Santa Eulalia.

Thompson has adopted a convenient terminology, calling the four potential names for New Year's Day Dominical Days and the actual New Year's Days complete with number the Year-Bearers, and calling the recurrence of the Year-Bearer within the same year but with a different number the Chief Day. A list is given of 105 sacred crosses in the neighbourhood of Nebaj at which ceremonies are performed every Dominical Day and more important ceremonies every Chief Day. Besides this prayers are made and

copal incense burned before the cross on the altar of every dwelling-house every Dominical Day. One may presume therefore that there must be a good deal of knowledge of the calendar even among those who are not professional keepers of the counts, as the occurrence of each public ceremony at such short intervals must surely help to keep the count in mind.

The count is the usual one of 260 days made up of 13 numbers and 20 names, and like all other modern counts it agrees day for day with those already discovered in modern times and with that of the old calendars, including the Aztec, but with the possible exception of Landa's count for Yucatan, though it seems more probable that, as suggested by La Farge, this was an error of one day by Landa. If so all the 260-day periods would agree exactly. The Ixil names are the Quiché ones and the Dominical Days are also the same as those of the Quiché. There is only one statement in the whole of this very valuable paper which I find it difficult to accept, namely (p. 106) that the counts of 13 numbers and of 20 day names are kept (in Chajul) by different priests who specialize in their respective counts. No further reference is made to this statement and nothing appears about it in the microfilm of Lincoln's notes: it stands quite isolated and unexplained. Certainly it is contrary to all other information on Middle American calendars, whether derived from modern investigators or the old Spanish sources or from the Maya and Aztec codices, all of which seem to regard the day-and-number count as a continuous one with the days and numbers inseparable from each other. La Farge confirms this (*Santa Eulalia*, p. 171).

Lincoln was unable to collect a satisfactory list of the 18 named months, but the fragmentary lists he secured have several names agreeing with those collected in other parts of the Maya field, although their proper order could not be established owing to failure of memory by the native priests. He made one discovery which is, so far, unique: namely, that at least one priest recognized the existence of a 13-year period when the Year-Bearer with the number of one will return 12 years later with the number 13, and this Year-Bearer number one is called in Spanish the *Presidente* and dominates the whole period. As Tche is the highest ranking Dominical Day, Lincoln suggests that 1 Tche was the starting point of a 52-year cycle, which would make such a cycle begin in 1506. In a note Thompson compares the Aztec cycle starting in 1507 with 2 Acatl. The day before this would be equivalent to 1 Tche in the Ixil count. Thompson further says that an early source which he cannot remember states that the 52-year Aztec cycle once started with 1 Tochtlil and was moved to 2 Acatl. The source he has in mind must be that commentary (in Kingsborough) on the Codex Tell. Remensis which says that Montezuma altered the year to 2 Acatl on account of the famines which for 200 years had always occurred in the years 1 Tochtlil.

One may amplify Thompson's note by adding that the Jacalteca according to La Farge (*Year-Bearers' People*, p. 159) consider the day K'anil which is the equivalent of the Aztec Tochtlil to be the chief Year-Bearer.

Lincoln gives examples of the method of divination by the calendar and describes the status of calendar priests and the personal days which they must all have. He describes also the very important ceremonies at the second recurrence of the Year-Bearer in the year and the remarkable and unique rotation of the ceremonies in four years through the world direction crosses.

RICHARD C. E. LONG

## ASIA

**Hindu Religion, Customs and Manners: Describing the Customs and Manners, Religion, Social and Domestic Life, Arts and Sciences of the Hindus.** By P. Thomas. Bombay (Taraporevala), n.d. Pp. xiii, 161, 251 illustrations. Price Rs. 22.

**54** From the sub-title of this book it is at once apparent that the author has set himself a formidable task, and it is hardly surprising that his attempt to compress into just over 150—admittedly very large—pages a description of Hindu culture from Vedic times to

our days is only partially successful. What Mr. Thomas describes is not so much Hindu custom as observed in any particular part of India at a given time, but an abstraction of Hindu custom as it is—or should be—according to the scriptures. In the treatment of the caste system, for instance, the author accepts the traditional fourfold division in its most static form, and devotes six pages to the Brahmins, while allowing only one to the Sudras, even though he states that 'they are more numerous than all the three Aryan communities put together and form the bulwark of Hinduism.' As



K. M. Panikkar has already pointed out, this fourfold division is little more than a systematization on a horizontal basis of a very complex racial and cultural whole and we have no historical proof for the one-time existence of a society which would correspond to the ideal view of Hindu thinkers. In the 16 pages devoted to 'Religion' the author had obviously no other choice than to give a brief descriptive account of the main sects and of the accepted beliefs of orthodox Hinduism, and this chapter, like others, suffers from the difficulty of combining with a reproduction of the traditional views an adequate indication of historic developments through the ages.

The chapters on 'Social Life' and 'Domestic Life,' on the other hand, deal almost entirely with present conditions among urban classes, and where the author allows himself some latitude in depicting details of social behaviour on specific occasions, his descriptions gain considerably in value and contain some very illuminating observations. 'Literature,' 'Music and Dancing' and 'Architecture, Sculpture and Painting' receive brief treatment in a predominantly historical fashion, and practically no attempt is made to analyse their position in modern times. The same applies to the chapter on the 'Art and Science of Love,' which contains a good many quotations from classical literature, but leaves one with the impression that present-day India offers comparatively little scope for that sophistication of sexual love which was so characteristic a feature of mediæval Hindu city life.

At a time when serious anthropologists and psychologists write in a more or less impressionistic manner about 'the Americans' or 'the Russians,' it should also be possible to give an equally cursory and yet telling picture of the Hindu way of life. But the author has complicated his task by extending his descriptions over a span of nearly 3,000 years and we can only admire him for producing nevertheless a readable and not too cumbersome book. While it does not claim to be an original contribution to learning, it can help the general reader to clarify his ideas on India, and even the western anthropologist visiting India for the first time may find it a useful introduction to various aspects of Hindu culture, though

he will hardly agree with the author's views on the early relations between 'Aryans' and 'Dravidians' expressed on pp. 3 and 4.

C. VON FÜRER-HAIMENDORF

### The Matrilineal Social Organization of the Nagas of Assam.

By K. M. Kapadia. Bombay (Popular Book Depot), 1950. Pp. 34. Price Rs. 1.

**55** This essay by the Reader in Sociology at the University of Bombay seeks to establish, by means of a very naive application of the method of W. H. R. Rivers, that the Western Nagas (Angami, Rengma, Sema) formerly possessed a matrilineal descent system. In his concluding paragraph the author recognizes that there are methodological objections to many of his arguments but challenges any would-be critic to provide an alternative explanation for the features of Naga social structure to which he draws attention. I do not feel that this would be very difficult. I suggest as a start that Dr. Kapadia consider what Professor Lévi-Strauss has to say on this subject in Chapter XVII of *Les Structures élémentaires de la Parenté* (1949).

E. R. LEACH

### Thailand : Nature and Industry : Ethnology. By A. Kerr and Erik Seidenfaden. Bangkok (Dept. of Commercial Intelligence, Ministry of Commerce), 1950. Pp. 37, 5 plates.

**56** This is a revised reprint of a chapter in the official commercial handbook of Siam first published in 1930. The type of information provided is similar to that given in Part 2 of W. A. Graham's *Siam* (1924). For most purposes Graham's account, with its superior illustrations and more orthodox spelling, will be preferred, but several culture groups are mentioned here which do not occur in Graham's scheme of classification. The authors seem to have taken considerable trouble to adopt a different spelling from Graham whenever possible. The Kaw become Ko; the Kawi become Kui; the Lishaw become Lissu; the Muhsö become Musso and so on. Furthermore there is no map. But perhaps, if it were not made unnecessarily difficult, ethnology of this kind would be rather dull.

E. R. LEACH

## EUROPE

### The Irish Countryman: An Anthropological Study. By Conrad M. Arensberg. New York (Peter Smith), 1950. Pp. xi, 216. Price 83.

**57** First published by Macmillan in 1937, the re-issue of this well written, sensitive and rewarding interpretation of Irish peasant life establishes this work as a minor classic of anthropology. The essays illustrate the advantages and some of the limitations of the functionalist approach criticized in the previous review (*MAN*, 1937, 231) as justification 'by works, if not by faith.' None the less, this reprinting is a welcome sign of the growing interest in anthropology.

J. M. MOGEY

### The Lappish Dialect of Jukkasjärvi: A Morphological Survey. By Björn Collinder. Uppsala (Almqvist & Wiksell) 1949. Pp. xii, 304. Price 15 kr.

**58** The Lappish dialect treated in this thesis by Professor Björn Collinder of Uppsala is spoken in one of the northernmost districts in Sweden. It must be considered as mainly belonging to the Norwegian-Lappish group of dialects, but in certain aspects it also shows a close connexion with the Lule-Lappish group of dialects. Whereas the Norwegian-Lappish dialects proper as well as the Lule-Lappish ones are already well known through the investigations of several scholars, descriptions of the transition dialects have been missing. This study by Professor Collinder is therefore a very welcome supplement to the dialect investigations already published.

As indicated by its sub-title, the book gives a description of the morphology of the Jukkasjärvi dialect. This description is principally synchronous, but at several points the author also shows the historical background of present-day forms. On the whole the book constitutes a larger contribution to the field than descriptions of dialect morphology usually do. Thus several leading questions are discussed in an introductory chapter: the notions of word, suffix, case, etc.

Of great value for the further study of the Lappish dialects is the last chapter but one, on features of dialect geography. The book concludes with a section of texts with English translations. The main chapter, the morphological survey, covers about 200 pages, and among other things gives an excellent survey of sound alterations in word stems, where the consonant gradation, which is of such importance to the morphology of the northern Lappish dialects, is elaborately treated. This chapter is followed by an extensive collection of paradigmata.

The material is rendered in its original form, without any attempt to normalize it. This method has its weak points, but it is reliable, and mistakes resulting from normalization are avoided. The fact that the author never conceals any unevenness in the records gives one a convincing impression of his integrity. I should like to raise an objection on one point: the characterization of the occurrence of the weak grade in open syllables and the strong grade in closed syllables as 'anomalous distribution of grades.' As the dependence of the stem consonants on an open or closed following syllable, respectively, ceased to exist long ago, I find this a little fallacious.

This investigation by Professor Collinder is altogether of great value to the study of Lappish dialects. Everywhere we can perceive the sure hand of the specialist, and the presentation shows independence and originality as always with this scholar.

ASBJÖRN NESHEIM

### Sculpture della Sardegna Nuragica. By G. Pesce and G. Lilliu. Venice (Alfieri), 1949. Pp. 42, 48 plates. Price 15s.

**59** This attractively produced and inexpensive work is mainly the work of Giovanni Lilliu, who provides a short account of the bronze figurines of Sardinia, a catalogue, and sixty-four photographs of figurines all in the Cagliari Museum. The photography and reproduction are good, and for the illustrations



alone this small book is an essential for all archaeological libraries. We have not hitherto had available in such a form any extensive collection of the Sardinian figurines and we must be grateful to Dr. Lilliu for the care and research which have gone into the production of this book. Gennaro Pesce contributes a short sketch of Sardinian prehistory with special reference to the nuraghi. While not denying

that some nuraghi were built before 1,000 B.C., Pesce emphasizes that the *floruit* period of nuraghic construction was from 700 to 300 B.C. These famous bronze figures—many of which have very considerable artistic merit—date from this main nuraghic period, and form a remarkable element in the art history of the Western Mediterranean in the first millennium B.C. GLYN E. DANIEL

## CORRESPONDENCE

**The Dentition of the Australopithecinae.** Cf. MAN, 1951, 37 and 38, and also 41 in this issue

60 SIR,—I am grateful to Professor Zuckerman for pointing out the limitations of the biometrical comparisons of the teeth of the fossil Australopithecinae in the paper by himself and Mr. Ashton. This, I feel sure, will help to dissipate the misleading impression which others besides myself (as I can affirm from personal discussions) have gained from reading this paper. There is one point, however, which I feel needs to be emphasized rather strongly. The authors state that their purpose was to compare their measurements of the teeth of anthropoid apes with those published by the workers responsible for the description of the fossils. I fear, however, that in some important instances these comparisons are not valid. The most obvious example is the antero-posterior length of the canine. In the canines of anthropoid apes the maximum antero-posterior length is at the base (since the tooth has a conical, tusk-like crown). In all the Australopithecine canines (deciduous or permanent) of which I have information at present, the crown is spatulate as in man, broadening out from the base and then rounding off to a relatively blunt point; the maximum antero-posterior diameter is therefore some distance above the base of the tooth. Ashton and Zuckerman carefully define the maximum antero-posterior length of the canine in their measurements of apes' teeth as the distance from the most anterior to the most posterior point on the enamel line at the base of the tooth (though it should be noted that the absolute maximum is usually slightly distal to this, at the level of the basal cingulum). But, unfortunately, they then proceed to compare this with an antero-posterior dimension at about the middle of the crown in the Australopithecine teeth, which, of course, is a very different measurement. In other words, it seems that, because the canine tooth has quite a different shape in the apes and the South African fossils, the authors have fallen into the trap of comparing two dimensions which, on their own definition, are not comparable, and then, as the result of this false comparison, they conclude that the shape is the same. Thus, in reference to the permanent lower canine of *Plesianthropus*, they state that 'the shape of its crown is similar to that of all existing apes.' I feel that it is desirable to call urgent attention to this sort of fallacy, for it serves to emphasize that, in applying modern statistical methods to such morphological comparisons, these methods do need to be used with care and circumspection.

W. E. LE GROS CLARK

Department of Human Anatomy, University of Oxford

### Note

Professor Le Gros Clark's letter has been shown to Professor Zuckerman, who makes the following reply:

61 SIR,—Care and circumspection are certainly needed when applying modern statistical methods to morphological comparisons. Had these methods been used at all in Professor Le Gros Clark's studies of the Australopithecinae, and in the original memoirs which give the overall dimensions of the teeth, this correspondence would almost surely not have occurred. If imaginary fallacies are not to be conjured up now, care and circumspection are equally necessary in other aspects of the work. Mr. Ashton and I have defined the maximum antero-posterior length of the canine as the distance from 'the most anterior to the most posterior point on the enamel line as seen from the labial aspect' (E. H. Ashton and S. Zuckerman, *Phil. Trans. Roy. Soc., Ser. B, Vol. CCXXXIV, 1950, p. 471*). If Professor Le Gros Clark inferred from this that we meant the lowest level of the line of junction of the enamel with the neck of the tooth, which is what I understand him to mean by the word 'base' (a word which is not in our definition) he was mistaken,

as he would himself have seen had he referred to our figs. 1 and 5. In these, the only two ape canines we illustrate, the levels of the maximum antero-posterior diameters are marked, and contrary to Professor Le Gros Clark's generalization about the shape of the ape canine, and to his interpretation of our statement, they are about two-fifths of the distance along the labial heights from the base. In this respect the difference, if any, between these two teeth of extant apes and those of the Australopithecinae seems to be one of degree, not kind.

No apparent scientific purpose can be served by speculations about a possible lack of identity in the dimensions we are comparing. This is a question of fact which could be rapidly settled if the original fossils or casts were made available for study. S. ZUCKERMAN  
Department of Anatomy, University of Birmingham

**Social Anthropology: Past and Present.** Cf. MAN, 1950, 198, 254 and 271 and 1951, 33, 34, and 35

62 SIR,—I am impressed by Professor Daryll Forde's cogent re-statement of the aims and methods of modern social anthropology, but I do not follow some of his criticism of Professor Evans-Pritchard. Forde says that the question of man as an automaton is only a relevant problem for quite other doctrines. I should have thought that it was highly relevant to the present issue inasmuch as Evans-Pritchard's objection strikes me as both methodological and philosophical. I think that it amounts to this. Either one accepts the scientific approach as the basis of one's methodology or one does not. In the former case, i.e. if one's conceptualization is 'scientific,' one tends to proceed in terms of either an organic or a mechanistic conception of society. To do this consistently one must also be prepared to accept the organic and mechanistic conceptions as something more than mere simile, otherwise one's scheme lacks logical substance; hence the need to reduce man to a variable and to regard him as an automaton. But in the last analysis it is with human beings, not with biological entities that can be reduced to abstractions, that one is dealing. It may be conceptually convenient to pretend that the behaviour of human beings is as instinctual as that of ants or bees, but in actual fact it is not. Human behaviour is based on intelligence. Intelligence implies flexibility, choice and indecision.

I am glad also to see Professor Evans-Pritchard pointing out that the fact that the anthropologist's problems are generally synchronous while the historian's problems are generally diachronic is a difference of emphasis and not a divergence of interest. This I take as an implicit reminder that in society the anthropologist is dealing with dynamic phenomena. The necessity of having to reconstruct non-literate cultures without regard to their history has tended to obstruct the methodological implications of this point and the result can be seen in the vogue of special concepts like 'social change,' 'culture contact,' etc. Yet, one has merely to turn to modern industrial society, for example, to find a rate of transformation which owes very little to culture contact as classically defined, but is at the same time sociologically more impressive than many of the instances of Western impact upon primitive societies. Once we get away from the static view we see immediately that social phenomena at any given point of time have to be conceived for methodological purposes as part of a social continuity. It is this awareness of social or historical continuity which should ally the anthropologist with the historian, and I agree entirely, therefore, with Professor Evans-Pritchard that the approach to the functioning of institutions includes studying both their past and their future.

Department of Social Anthropology,  
University of Edinburgh

K. L. LITTLE







(a)



(b)



(c)



(d)



(e)



(f)



A RADAY GIRL UNDERGOING THE TOOTH-SAWING OPERATION

Photographs: The Rev. G. H. Smith



# THE TOOTH-SAWING OPERATION AMONG THE RADAY OF FRENCH INDO-CHINA\*

by

THE REV. G. H. SMITH

*Banméthuot, Viet-Nam, French Indo-China*

63 I recently witnessed the operation of sawing off the six upper front teeth of a young girl of the Raday (or Rhade) tribe, at Buon Ea Sut, near Banméthuot on the Darlac plateau of south-central French Indo-China. The Raday are a large so-called Malayo-Polynesian group strongly influenced by the early Chams. The operation is performed upon both boys and girls, and is practised by many of the aboriginal tribes of Indo-China; some file their lower teeth to points. The custom, which is believed to make the children beautiful and ready for marriage, has no religious significance, but they are subjected to ridicule if they do not observe it. The operation usually takes place inside the long-house, but on this occasion was performed

become unendurable. In this case the aunt too was near at hand, sitting inside the house by a window. The uncle sat impassively throughout the operation and never once looked at the girl (Plate C*b*). Frequent stops were necessary to sharpen the saw (Plate C*c*) with a file; it was still too blunt, and slipped a good deal, and curved saw-edged paddy knives were also used. A piece of dirty rag was rolled up and placed between the girl's teeth; it was soon soaked with blood. In Plate C*d* the teeth can be seen half sawn through.

When the operation was finished the girl rinsed her mouth with warm water from an empty tin can. A stick of kray wood was brought and the end was burnt. The uncle collected the black lacquer which dripped from it on



FIG. 1. THE RADAY GIRL BEFORE THE OPERATION



FIG. 2. THE RADAY GIRL AFTER THE OPERATION

on the verandah so that I could take photographs. Anyone competent may operate, and is usually rewarded with a small payment.

The girl (fig. 1), who was about ten or twelve years old, seemed quite unperturbed by the prospect of her ordeal. She watched the operator, who had already performed on twenty people, make his preparations, which consisted of sharpening his saw (Plate C*a*). This was a piece of rusty hacksaw, not at all sharp. He did not wash the saw, and no sanitary precautions were taken. The girl then lay down on a mat, with her uncle sitting beside her. He replaced the girl's father, who was dead; the parents would normally be present to ensure that the child's suffering did not

an iron hoe blade. The girl smeared the lacquer on her gums and the stumps of her teeth (Plate C*e*, *f*), which cauterized the wounds and eased the pain. This was repeated twice a day for several days, and turned her teeth black. Apart from the injury to her teeth and gums the girl's cheek was cut at the side of her mouth (fig. 2). She showed no signs of pain, though no drug or anæsthetic of any kind was used during the operation, which lasted an hour and a half. Possibly the fact that she was fearless and relaxed partly explains this. Sometimes the children have to be held down, and often the pain is so severe that they run away into the woods for a day or two. The gums swell and abscesses often form later; occasionally patients bleed to death or go out of their minds.

\* With Plate C and two text figures



# THE CHRONOLOGICAL FRAMEWORK OF PREHISTORIC BARBARIAN EUROPE

by

GLYN E. DANIEL, M.A., PH.D., F.S.A.

64 In his review in MAN (1950, 190) of Professor Piggott's *British Prehistory*, Mr. T. G. E. Powell says, 'The chapter headings retain the old formularies of "Mesolithic," "Bronze Age," etc., but it is made clear in the text how these terms have been outgrown, and one may suspect that before long Professor Piggott will help to establish some new and more appropriate general terminology.' I do not wish to labour here a point which has already been discussed fully in the last fifteen years, namely the inadequacy of the divisions of the Thomsen three-age system as a cultural and chronological framework for the modern writing of prehistory.<sup>1</sup> My purpose in writing this article is to encourage discussion in MAN, for fifty years so hospitable to controversial issues, of the new formularies which must eventually replace the existing framework, which so many condemn yet find so difficult to replace.

## *Cultural and Chronological Categories*

Of course the new general terminology to which Mr. Powell refers must deal with two categories—one cultural and the other chronological. Here we are not primarily concerned with the cultural terminology; we may merely note in passing the earlier attempts of Menghin and Kendrick, and the current systems of Childe (*What Happened in History*, 1942) and Grahame Clark (*From Savagery to Civilisation*, 1946) with their attempts to wed the formularies of Tylor and Morgan to those of Thomsen and Gabriel de Mortillet. Indeed Tylor's definitions of Savagery, Barbarism and Civilization, and Morgan's seven Ethnic Stages may yet come back into general parlance through their rehabilitation by Childe and Clark; and I am here using the term 'Barbarian Europe,' though without much enthusiasm, in the sense in which these scholars re-define Tylor and Morgan, namely to include those peoples who succeeded in Western Europe the 'Savages' of the Upper Palaeolithic and Mesolithic, and who preceded the 'Civilization' of the Romans, Etruscans and Greeks, and who, in current archaeological parlance, are labelled Neolithic, Chalcolithic, Eneolithic, Bronze Age, and Early Iron Age, according to their technological achievements.

This problem of new cultural formularies must be kept separate from that of a new framework and terminology for the chronology of Barbarian Europe. The chronological problem is easy to adumbrate; it is the problem of obtaining a scale of chronological reference for the cultures and movements of those barbarian and savage societies who were contemporary with the civilized societies of the Near East and Aegean, some of whom were, from about 3000 B.C., calculating time in dated calendar years. It is not the problem of arguing absolute dates for events established relatively in Barbarian Europe

by stratigraphy, typology and association. It is the quite different problem of devising a framework of reference for events in a cultural sequence already established by methods of relative and absolute chronology. To put the problem in concrete form: it is possible to argue that the segmented faience beads found in Wessex round barrows, in the Parc Guren megalithic tomb, at the Grotte de Ruisseau and at Fuente Alamo all date from somewhere between 1350 and 1000 B.C.<sup>2</sup> How are we then to refer to cultural events in Western Europe that are roughly contemporary with these beads? Are we to refer to the last quarter of the second millennium B.C., to Early Bronze Age II, to Middle Bronze Age, to Middle Bronze Age I, to Bronze III, or to Mediterranean Bronze I? All such terms, and many others, are in use to describe the same period of time. It is this problem, I repeat—the problem of the chronological framework of reference and the terminology—that the new formulary must deal with, not the chronology itself.

We are not concerned here with the schemes of chronological reference for Savage Europe, i.e. Europe from the first appearance of man until the first peasant villages of the Neolithic. The chronological problems of the Palaeolithic and Mesolithic are very different ones which can be related to geological and climatic events and can even receive absolute dates by geochronological techniques. Our absolute dates for Barbarian Europe (in the Childe-Clark sense—the *protohistoire* of the French archaeologists) depend in the end on writing in the Near East. The same chronological formularies are unlikely to be necessary or appropriate for Savage, Prehistoric Europe as for Barbarian, Proto-historic Europe.

## *Present Systems*

There exist at the present day two systems of chronological reference for Barbarian, Proto-historic Europe. The first and most widespread system is based on subdivisions of the Thomsen three-age system, and all modern archaeologists have been brought up on divisions of the Bronze Age into Early, Middle and Late or into Periods I to VI or A to D, and of the Early Iron Age into Hallstatt A to D or I and II and La Tène I to IV, or into A, B, C as in Professor Hawkes's classic scheme for Great Britain or I to IV as in Dr. Raftery's scheme for Ireland. The difficulties of these schemes are well known to those who devise and use them; they are twofold. First, they remain both cultural and chronological divisions, that is to say they are not objective chronological schemes but essentially analyses of the surviving human material. As Childe has shown in his recent analysis of Reinecke's Bronze Age periods, these so-called 'periods' are in reality typological and cultural divisions.<sup>3</sup> Secondly, they are, from the point



of view of nomenclature, tendentious, in that, by referring to subdivisions of the technological stages of the Neolithic, Bronze and Iron Ages, they prejudice what might otherwise be thought to be chronological periods. We see this inevitable confusion in the attempts made by Mahr and Raftery to name the chronological periods of proto-historic Ireland during the second half of the first millennium B.C. Mahr speaks of a Late Bronze Age B and C,<sup>4</sup> while Raftery refers to the same periods as Iron Age I and II.<sup>5</sup> Why not chronological terms that refer neither to Bronze nor Iron?

The second system of chronological reference at present in use for Barbarian Europe is based on approximate dates in calendar years. It is the hope of every archaeologist that a time will come in the development of their studies when the use of exact dates will be practicable; that it may one day be possible to say 'the foreign stones of Stonehenge were brought from the Preseli Hills to Wiltshire between 1750 and 1700 (or 1740) B.C.,' but it must be admitted at present that that day seems far distant. When we read in Piggott's *British Prehistory* that 'at a date probably about 2600 B.C. simple agricultural communities were being established in Spain and southern France,' and that 'soon after 2500 B.C. we have the first agricultural settlements established in southern England,' these dates are put forward, as Professor Piggott himself emphasizes in his book, as working hypotheses. It should always be remembered that the adoption of such exact dates is a methodological device, and that these dates may well err by one or two hundred years each way.

At the moment then, these 'exact dates' in the Barbarian chronology of Europe of the second and third millennia B.C. are not so much real dates as a translation of a point of time in the relative sequence of cultures into a terminology which is readily understandable and of general reference. It is, in distinction to the use of subdivisions of the three-age system, objective, if only approximately correct. But can we carry this technique further at present in the proto-historic study of Barbarian Europe, and bring the approximate dates system into a marriage with the periods of the three-age system? I believe that we can. If there is such doubt about so many dates in the third and second millennia, if there is this bracket of doubt around each date, then what we are doing is *already* dating cultures and objects to periods and not to exact dates. It seems likely, then, that our much needed new scheme of chronological reference should recognize this fact and should be, not in terms of exact dates (which are anyway not exact) nor in terms of 'periods' (which are anyway not chronological periods but subdivisions of an outworn cultural and technological sequence) but in terms of approximately dated periods.

#### *Childe's System of Periods*

Professor Childe faced the problem of a new chronological system when, in the 'twenties, he wrote *The Dawn of European Civilisation* (1925) and *The Danube in Prehistory* (1929). In the first edition of *The Dawn* he refers to a Period I (the first half of the third millennium B.C.), a

Period II (the second half of the third millennium B.C.), a Period III (his map of this shows Europe at about 2000 B.C.), and a Period IV (the map of which shows Europe in about 1600 B.C.). In *The Danube in Prehistory* seven periods are more exactly defined as follows:

Period	I	to 2600 B.C.
	II	2600-2300 B.C.
	III	2300-1800 B.C.
	IV	1800-1600 B.C.
	V	1600-1300 B.C.
	VI	1300-900 B.C.
	VII	900 B.C. onwards.

And in the current edition of *The Dawn* the same first four periods are used but with the following dates:

I	to 2200 B.C.
II	2200-2000 B.C.
III	2000-1700 B.C.
IV	1700 B.C. onwards.

As far as I can make out, Childe has nowhere set these periods out as a new chronological system for European Barbarian archaeology; they merely emerge from his works as a new system of reference.

In dealing with British prehistory, however, Childe has consciously set out a system of dated periods, and in his *Prehistoric Communities of the British Isles* (1940) has devised a system of nine periods labelled I to IX. These are not dated exactly, but it is clear from the text that 1400 B.C. is provisionally used by him to divide Periods IV and V, that Period VI begins at 750 B.C. and that Period IX starts at 75 B.C. This system of nine periods for Barbarian Proto-historic Britain is certainly one of the most important advances ever made in the methodology of British prehistoric archaeology, but although first set out over ten years ago, it is seldom used in scholarly articles of popular books. This is most unfortunate. It seems to me that the time has come for a very careful re-examination of the scheme of chronological reference implicit in *Dawn* and *Danube* and deliberately set out in *Prehistoric Communities*. I think they have in them the germ of as great a change in prehistoric methodology as was the Thomsen three-age system in the twenties of the nineteenth century, or the replacement of the epochal-geological categories of de Mortillet by cultural categories in the early years of the twentieth century.

#### *Objective Chronological Periods*

The hope for the Barbarian chronology of Europe for some time would seem to me to lie in approximately dated periods; but these periods must differ in one important respect from those set out by Childe. Professor Childe, with his numbered periods, has got over one of the difficulties of using the three-age system, namely the tendentious nomenclature. By using his system we can refer to cultures and objects in early Britain as belonging to his Periods II or III without begging questions as to whether they are 'Neolithic,' 'Chalcolithic' or 'Early Bronze Age,' and I have found this system most convenient to use in my *Prehistoric Chamber Tombs of England*



and Wales (1950). But Childe's system preserves the other difficulty, namely that his periods are still both cultural and chronological. Indeed he himself admits that 'these periods are defined by cultures, expressing the social traditions of peoples.'<sup>6</sup> Professor Childe has taken one great step forward: we need, it seems to me, to take one more.

The process of emancipation from the fetters of the old chronological formulæ is twofold; we must get rid of the tendentious names, and we must, secondly, get rid of the cultural ties, and so become objective. Childe has taken us through the first stage; we need to follow him and go one stage further. What is now needed, I suggest, is an agreed framework of periods that are chronological periods, and not at the same time cultural periods, and

Period II had different dates, but that the cultural phase described as Danubian II now belonged, not to his chronological Period II, as originally, but to his chronological Period III.

It is in the end, as stated at the outset of this article, the splitting of cultural and chronological periods. But before this can be done satisfactorily we need an agreed framework of dated periods from 3000 B.C. onwards. Two such frameworks are set out in fig. 1 for comparison with the current schemes of reference in use in Southern England (taken from the Council for British Archaeology's *Survey and Policy of Field Research* (1948)), in Ireland (based on O'Riordáin, Mahr and Raftery), in France (based on modifications of Déchelette), and one of the systems now

	S. England	Ireland	France	Spain	Scheme A	Scheme B	
3000							3000
					1	I	
2500					2	II	2500
			Neolithic	Neolithic	3	III	
2000	Neolithic	Neolithic	Copper or Bronze I		4	IV	2000
					5	V	
1500	Early Bronze	Early and Middle Bronze	Bronze II	Mediterranean I	6	VI	1500
			Bronze III	Bronze II	7	VII	
1000	Middle Bronze		Bronze IV	Atlantic I	8	VIII	1000
				Bronze II	9	IX	
500	Late Bronze I	Late Bronze A	Hallstatt	Celtic and Iberian I	10	X	500
					11	XI	
	Early Iron A	Iron I	La Tène		12	XII	
BC AD					13	XIII	BC AD
					14	XIV	
500	Roman		Roman				500

FIG. 1. COMPARATIVE TABLE OF PREHISTORIC SYSTEMS

into which, and against which, the cultural phenomena studied by the archaeologist can be fitted, *without altering the framework*. This would make the periods objective chronological periods and would provide a firm system of reference even when cultures and artifacts may change their relative and absolute dates. Already Childe's European (or Danubian, as it was originally) Period II has changed in a quarter of a century from 2600-2300 B.C. to 2200-2000 B.C. But all this means really is that the date of Phase II of the Danubian culture has been scaled down; no more than that. We need a system of periods that do not change when cultural datings change. That is to say, if Childe had retained his original *Danube in Prehistory* scheme of periods he would now be saying, not that

in use in Spain (that of Santa-Olalla in his *Esquema Paleontológico de la Peninsula Hispanica* (1946)). Scheme A divides the time concerned into quarter-millennia numbered 1, 2, 3, etc.; Scheme B divides it into periods of three hundred years numbered I, II, III and so on. The first four columns are merely for cross-reference and the two schemes are set up only as Aunt Sallys. It should, however, be said that in teaching some aspects of the proto-history of the Western Mediterranean and France in the Department of Archaeology and Anthropology at Cambridge during the last five years, I have found the second scheme (Scheme B) of three-hundred-year periods to have very considerable advantages in exposition. It is possible, for example, to ascribe the horizon of the faience beads in the Wessex



round barrows, at Parc Guren, at the Grotte de Ruisseau, and at Fuente Alamo to Period VI or VI/early VII and for this to convey something tangible and intelligible to the student at once, without recourse to phrases such as those mentioned earlier, or to actual calendar dates of dubious exactness. And of course if it is subsequently found that the faience beads really date from Period VII, the system remains intact. The chronological framework remains, if the relative cultural sequence changes or new absolute dates are proposed.

### *An Agreed Scheme*

But of course such an extension of the Childe system of periods into a framework of objective dated periods must remain at present only as a form of personal shorthand for teaching and research, and as such, and to stimulate discussion, it is mentioned here. One cannot have the new formulary which so many deem necessary and urgent unless someone is prepared to put up the Aunt Sally and invite attack. It is hoped that some agreed system (not necessarily Scheme A or B) will soon be set up as the basis of a new chronological framework of reference. It need only be used by those who find it clearer and more helpful than the existing confusing systems. It would be a permissive framework of reference and would not be obligatory on archaeologists any more than it was obligatory for archaeologists a hundred years ago to abandon using terms like Ancient British and Gothic and Druidic and adopt the Stone Age, Bronze Age and Iron Age. It would justify itself, as the three-age system did for so long, if it were found to be a useful conceptual and methodological tool at the moment.

But such a system of numbered periods would have to be generally agreed, even if not generally used. It would only add to our existing difficulties if archaeologists in different countries, and different archaeologists in the same countries, devised varying systems of periods. The Eighteenth International Conference of Orientalists at Leiden in 1931 discussed the various Mesopotamian discoveries made since 1918 and agreed to distinguish three predynastic periods of Mesopotamian history; the household words al'Ubaid, Uruk and Jemdet Nasr were the

results of the agreement at this conference. It would be appropriate if some comparable body, say the International Congress of Prehistoric and Proto-historic Sciences at its 1954 meeting in Madrid, agreed on a system of chronological periods for use by such proto-historians working in Europe as wished to do so. A suggestion that discussion at an international conference is imperative to end what he describes vividly as the anarchy of current prehistorical nomenclature, has already been made by Professor Pericor in his *Nuevos ataques al sistema tradicional de la nomenclatura prehistorica*<sup>7</sup> and this suggestion deserves the warmest support.

Of course the most difficult problem will be nomenclature. Scheme A might use a convention based on quarter-millennia such as III/1, III/2, III/3, III/4 and so on, but we should get into some confusion when it was desired to distinguish between the last quarter of the first millennium B.C. and the first quarter of the first millennium A.D. This difficulty might not arise in France, Spain and lowland Britain, but it would in Ireland, north Britain and Scandinavia. It seems to me at this moment that the most convenient method is boldly to divide the period from 3000 B.C. onwards into an agreed series of approximately dated periods with an agreed numbering or lettering. But this, like the whole of this brief article, is no more than a suggestion to provoke discussion and perhaps to stimulate others to produce something better. Some such system of objective chronological reference is to my mind, without any doubt, the most pressing need of prehistoric archaeology today.

### *Notes*

<sup>1</sup> See particularly V. G. Childe, *Proc. Preh. Soc.*, 1935, pp. 1ff.; *Modern Quarterly*, Spring, 1946, pp. 18ff.; *J. Roy. Anthrop. Inst.*, Vol. LXXIV (1944), pp. 7ff.; and Daniel, *The Three Ages* (1940) and *A Hundred Years of Archaeology* (1950).

<sup>2</sup> On these dates see especially H. N. Savory, *Arch. Camb.*, 1948, pp. 79-82, and *Revista de Guimarães*, Vol. LX (1950), pp. 7ff.

<sup>3</sup> *Proc. Preh. Soc.*, 1948, pp. 177ff.

<sup>4</sup> *Proc. Preh. Soc.*, 1937, pp. 379ff.

<sup>5</sup> *Marburger Studien* (Darmstadt, 1938), pp. 202-4; *Fóil-Sgríbhlium Éóin Mhic Néill* (Dublin, 1940), pp. 272-81.

<sup>6</sup> *Prehistoric Communities of the British Isles* (1940), p. 9.

<sup>7</sup> *Ampurias*, Vol. VI (1944), pp. 291-2.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

### Ancient Mining and Metallurgy Committee: Examination of a Fragment of the Welwyn Bowl in the British Museum

65

The following technical report by Dr. Voce, and Mr. Watson's note explaining the object of the research, are of particular value in throwing light on a technique about which little information has as yet been available to the archaeologist. Indeed, until recently, the process of metal-spinning has been regarded as of comparatively recent origin.

Metal-spinning is a process by which circular discs of thin metal may be formed into bowls, dishes, beakers, etc. The forming or spinning is carried out by rotating the disc of metal at a relatively

high speed while considerable pressure is applied to the rotating disc by means of a blunt tool. It follows that some quite substantial kind of lathe must be available, and that the lathe must be provided with a spindle which will carry a former bearing the shape of the desired vessel; the metal being spun down over the former until it attains the finished shape. The former may be likened to the chuck of a modern lathe with the exception that the work is forced down over the former itself instead of being held, and turned, in the chuck by means of a cutting tool held in the tool post of the lathe. The operation is also different from normal lathe work in that the metal must be kept in a soft condition by means of frequent annealing. It is most satisfactory that the



technical evidence given by Dr. Voce furnishes us with scientific grounds for considering that by Belgic times in this country metal-spinning was in all probability being carried out in the lathe.

H. H. COGLAN

#### OBJECT OF THE RESEARCH

When the Belgic bronzes found at Felmersham, Beds, were published in the *Antiquaries' Journal*, Vol. XXIX (1949), pp. 37ff., it was not possible to quote more than the briefest metallurgical evidence in support of the theory that the bowls were manufactured by the spinning process, and the argument rested mainly on the external appearance of the fragments. There has been no opportunity subsequently of taking the scientific investigation of Felmersham vessels any further, but by the mediation of Mr. H. H. Coghlan, F.S.A., Chairman of the Ancient Mining and Metallurgy Committee of the Royal Anthropological Institute, a detailed analysis was made of a rim fragment of the large bronze bowl from Welwyn, Herts (see *Antiq. J.*, Vol. XXIX, p. 56), which approaches the Felmersham bowl so closely in shape, size and the external indications of the mode of manufacture as to suggest strongly that the two vessels came from the same workshop. The present report is the work of Dr. E. Voce, M.Sc., F.I.M., metallurgist of the Copper Development Association, the spectrographic analysis being carried out and photomicrographs prepared by the courtesy of the British Non-Ferrous Metals Research Association; to these and to Mr. Coghlan I am greatly indebted for adding scientific weight to my argument. Dr. Voce's report is given in full.

W. WATSON

#### REPORT

##### Micro Specimen No. 45, Analysis No. 2

The following is the result of the spectrographic analysis of the submitted fragment of the Welwyn bowl:

Tin	present
Antimony	~0.2 per cent.
Arsenic	0.03 "
Bismuth	0.003 "
Cobalt	0.005 "
Iron	~1 "
Lead	~1 "
Phosphorus	not detected
Silicon	0.2 per cent.
Manganese	trace
Nickel	0.01 "
Silver	~0.01 "
Zinc	not detected
Tellurium	not detected
Aluminium	trace

~ = probably about.

> = more than.

~> = probably about...or more.

Where tin is stated to be present the alloy is a bronze containing at least 0.5 per cent. and possibly 5 per cent. or more of this element.

The analysis shows this to be a tin bronze containing considerable amounts of iron, lead and antimony.

The basic structure was that of an annealed  $\alpha$  solid solution of very small and uniform grain size. As usual in lightly cold-worked bronze, slip bands were present on the crystals, but the crystal boundaries and twin boundaries showed little or no distortion, indicating either that the metal had been worked at a temperature above that of recrystallization or had been cold-worked and subsequently annealed, followed, in either case, by sufficient cold

deformation to produce slip bands. The structure is illustrated in fig. 1, which also indicates cracking, to be described later.

A homogenizing heat treatment at about 700° C. would be necessary before material of this composition could be worked either hot or cold.

Though inclusions were prevalent, the great majority of them appeared to be lead, and few if any showed the clear blue colour of iron. This suggests that relatively high temperatures were employed for working or for annealing and that they were followed by sufficiently rapid quenching to retain most of the iron in solid solution. The absence of clearly recognizable iron particles is strange in view of the analysis. Iron in solid solution tends to restrain grain growth and might account for the small grain size of the alloy.

Except in a sample taken from near the edge of the bowl, the inclusions were neither elongated nor arranged in stringers, and even at the edge position elongation of the particles was not very marked. This indicates that the deformation during fabrication

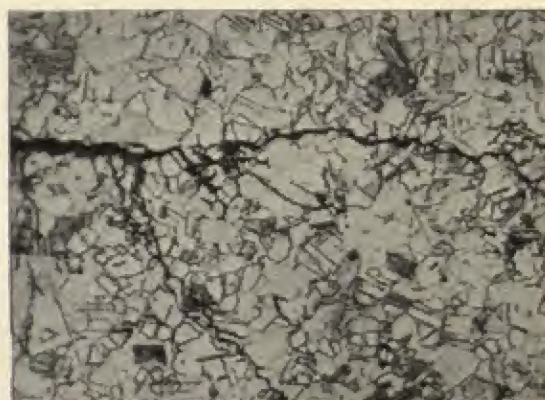


FIG. 1. MICROPHOTOGRAPH OF FRAGMENT OF THE WELWYN BOWL IN THE BRITISH MUSEUM ( $\times 300$ )

The figure shows very small twinned crystals of annealed  $\alpha$  bronze with slip bands and intercrystalline cracking.

was not unidirectional. It is probable that a flat or dished cast plate of bronze was homogenized by heat treatment and fabricated into bowl form by hammering either while hot or after quenching. If it was not worked hot, there must have been several intermediate anneals, leaving the bowl finally in the annealed condition. As  $\alpha$  bronze is not very easy to fabricate hot, it seems probable that the method adopted was that of alternate cold working and annealing.

Circumferential markings on the larger of the fragments examined showed that the bowl had been finished by a rotational process akin to spinning or lathe turning. It is possible that the circumferential markings were due to the use of a planishing stone or similar implement rubbed round the bowl. The markings were more prevalent on the inside than on the outside of the bowl, except at the flange where they disappeared from the inside and occurred on the outside, which, however, is the inside of the bend in this region. Thus in the spinning or planishing operation the metal appears always to have been pushed outwards by the operator, possibly against a female former. The speed of rotation, if any, was probably quite slow, since many of the marks deviated considerably from a circumferential path. Possibly the bowl was fixed to something like a potter's wheel rotated by hand by an assistant.

There were numerous cracks, and while some of them were



parallel to the circumferential tool marks many were perpendicular to these. The cracks were possibly attributable to internal stresses in the metal, which, though the material had been annealed, may have arisen during the planishing or other finishing operations. It has already been pointed out that slip bands were visible in the crystals, indicating a certain degree of cold deformation. The cracks showed the typically intercrystalline path of stress corrosion cracking, and considerable corrosion of the intercrystalline type with ramifications along crystallographic planes had occurred. Many of the cracks emanated from such corroded regions.

At one point in the larger of the specimens examined there was an oval patch of discoloured metal more or less outlined by cracks. This appeared to be a casting defect. Its longer axis was perpendicular to the circumference of the bowl, suggesting that

the main deformation during fabrication was in the radial direction.

To summarize, it seems probable that the bowl was fabricated from a cast plate or dished casting by homogenizing heat treatment at about 700°C., followed perhaps by hot forging in the early stages (though even under modern control the hot forging of  $\alpha$  bronze is not particularly easy) and subsequently by alternate cold working and annealing, with sufficiently rapid cooling to retain the iron in solid solution. It was clearly finished, if not fabricated, by a circumferential planishing, spinning or turning operation in which the tool was applied to the concave surfaces. These suggestions are in accord with Watson's description of 'Bowl No. 1' (*Antiq. J.*, Vol. XXIX, 1949, pp. 42f.) and would account for the observed regular diminution of thickness from the rim towards the centre.

E. VOCE

## REVIEWS

### AFRICA

**East African Background.** By G. W. B. Huntingford and C. R. V. Bell. London (Longmans), 1950. Pp. 124. Price 7s. 6d.

66 This little book was originally written to provide Europeans serving in the East Africa Command during the last war with some knowledge of the character and background of the African personnel with whom they would be thrown into daily contact, and it has now been revised and republished as a general introduction to the ethnology of East Africa. It was designed admirably to fulfil the former essentially practical function: it is, unfortunately, less adequately adapted to the latter. This is perhaps to some degree inevitable; what the average European soldier, with no professional interest in anthropology, needs to know in order to deal fairly and intelligently with his African N.C.O.'s and men is one thing; what the student, seeking a coherent and accurate picture of East African tribal organization, political and social structure and ethnographic affiliations, requires is something quite different. Emphasis on the necessity for consideration and tact in dealing with Africans and on the desirability of learning their languages, and practical hints as to what to do if, for example, an African for whom you are responsible believes himself to be bewitched, are useful if not essential in one context, redundant in the other. Certain of the authors' broad generalizations on African character, e.g. 'the sense of responsibility is rather markedly lacking in the African' (p. 114), may indeed be considered gratuitous in either.

The book contains, *inter alia*, an introductory account of East African history and prehistory, a number of chapters on magic, religion and witchcraft, kinship, primitive economics, warfare, etc., and a list of the tribes of East Africa, with brief but informative accounts of the political organization of a few of them. As might be expected from Mr. Huntingford, the account of the Nandi is precise and illuminating, and there is a useful description of the age-set system of this tribe.

It is a pity that the considerable space given by the authors to theoretical discussion and interpretation was not devoted to further descriptive material, for many of their anthropological *dicta* will hardly be accepted by most students of the subject. The African's belief in magic, for example, is not adequately explained by his 'undeveloped powers of thought' and inability to understand the relation between cause and effect (p. 29), nor can the existence in an African tribe of 'a man who appears to be a chief, but who is really no more than a religious head with no executive or judicial functions,' be attributed simply to the fact that the African 'often likes the outward semblance of chieftainship' even in default of its reality (p. 14). Even on the descriptive level, the consecutive statements that 'in times past the Kabaka [of Buganda] was an absolute king' and that 'his chiefs in their turn were absolute in their power' (p. 25) can hardly pass unchallenged; they are evidently mutually incompatible, to say nothing of their relation to the political reality.

The general guide to tribal distribution, augmented by a map, is useful, but in the present stage of our knowledge of the ethnography

of the area it inevitably suffers from some inaccuracies. A slip on p. 97 requires correction; the Sukuma Districts are of course south, not north, of Lake Victoria.

The foregoing criticisms should not be regarded as invalidating the book's usefulness within the limits of its original intention; it contains a good deal of valuable information, and will provide for the layman whose work or interest centres in East Africa an interesting and instructive guide. That there is a need for a simple and easily read book of this kind cannot be doubted.

J. H. M. BEATTIE

**The Lamba Village: Report of a Social Survey.** By J. C. Mitchell and J. A. Barnes. *Comm. Sch. Afr. Stud. N. S.*

67 No. 24. Cape Town, 1950. Pp. vi, 69

Apart from its contribution to our knowledge about the Lamba, this report invites comment on a method of training in 'basic techniques of field research.' In 1946 the new research officers appointed by the Rhodes-Livingstone Institute were taken into the Lamba region by the Director, where they worked as a team for about three weeks. This report is the result of their labours, and the fact that it claims to be only a by-product of an essay in training fieldworkers adds to its interest.

The bulk of the report contains the results of the survey. It gives demographic data for 16 villages; the clan system; the lineage structure; types of marriages and divorce rates; estimates of labour migration, and cash budgets of 29 households. Considering the short time spent in the field, the data amassed are impressive.

Professor Gluckman's introduction contains a critical summary which he made in 1947 as Director of the Institute. Then in 1949, in the light of their later research, he amends his own views. So we are permitted to see something of the training of fieldworkers at three stages.

For a rapid social survey, the problem is to know in advance what categories of information will be valid and enlightening. The team, in making an analysis of the kinship composition of villages, classified each person in terms of membership of lineage groups, related to that of the headman. In his introduction Professor Gluckman gives a useful list of the attributes of corporate lineages found in Africa, and distinguishes these from the type of groupings which are characteristic of the Lozi, Tonga and Bemba. He goes so far as to say that the Central African kinship systems may be defined by the absence of the corporate lineage. On the basis of this distinction, he suggests that the analysis of Lamba village composition should have been made in a different set of categories, which would have demonstrated the relation of the Lamba kinship system to those of other typical Central African peoples. These suggestions were made, of course, in order to clarify problems for future research. In 1949 he added another note to say that the criticism had been so fruitful that subsequent research had probably destroyed its application to the Lamba. Mitchell had found among the Yao that the villages were actually composed of small corporate



lineages, linked to one another by varying ties of kinship with the headman. As his analysis of the Yao might well turn out to be equally valid for the Lamba, the original classification chosen by the team would be nearer the mark than that afterwards suggested by the Director.

It would not be fair to judge this report only by the value of the results embodied in it. On this basis one must agree with the Director's opinion that too much stress is laid on quantitative calculations; but as he was working with the team, he must have been partly responsible for this bias.

The survey should also be judged by its value in the training of fieldworkers. On this count the excellence of the later research by individual members would seem amply to justify it. But one is tempted to ask whether in fact the three weeks spent on practising the methods of extensive survey were really very helpful to them, engaged later as they were on intensive investigation. The latter requires a different approach, and the techniques of rapid survey might even hinder an investigator from establishing the social relations necessary for intensive work. It may be asked whether the period of study at Cape Town may not have been as useful as the survey of Lamba villages in the training which produced the later research on the Ngoni, Yao and Shona.

MARY TEW

**Die Bambuti-Pygmäen von Ituri, Vol. II: Ethnographie der Ituri-Bambuti, Part III: Die Religion.** By Paul Schebesta, S.V.D. *Mém. Inst. Roy. Col. Belge, No. IV, Part 1.* Brussels, 1950. Pp. 253, 4 plates.

68 Father Schebesta characterizes Bambuti religion as a dynamic one, that is, he considers that the 'supreme being' of that religion is the concentration of the vital forces (*Lebenskraft*) which emanate from that being to give life to man and, to a lesser extent, society. Magical strength, *megbe*, is thus intimately connected with this being, and magic falls within the same conceptual frame as religion. The 'supreme being' is, however, very remote, the various characteristics ascribed to him being hypostasized mainly in the moon and forest divinities and, less importantly, in the beings connected with the dead. It is to the forest divinity that Bambuti prayer and sacrifice are directed, as this being is thought to control their means of livelihood.

Father Schebesta relies heavily on etymological evidence in his analysis. On the hypothesis that the Bantu word root is disyllabic, beginning and ending with a vowel, with the initial consonant as a primary prefix and the 'class' affixes as secondary, he finds that the three terms for 'divinity' most widespread in Africa meet in Ituri: from the root *amba* (*aba*) giving *amba*, *dzumbu*, *kyumbu*, etc.; from *ara* giving *mbali*, *tore*, *kulu*, etc.; from *anga* (*angwe*), *aga* (*agwe*) giving *mu(n)gu*, *mu(gwe)*, *kahinga*, etc. The root *ada* (*anda*) he associates primarily with magical concepts. (These word lists cover the Efe-Lese and Madi-Balundu groups.) The linguistic analysis, towards which Father Schebesta here presents only that material relevant to Bambuti Pygmy religion, is in the tradition of comparative Indo-European linguistics. A discussion of the particular merits of his hypothesis must be left to those more versed than I am in African and Bantu linguistics. Whether and to what extent etymological data should be used for ethnological reconstruction has been hotly debated ever since Max Müller used this method with rather less caution than imagination. Meaning is perhaps more satisfactorily derived from cultural context.

Whatever value the reader may wish to attach to this portion of the volume the remainder must be praised for its descriptive material, for the careful analysis of Pygmy-Negro cross borrowing, for a well documented comparison with Bushman religion, and for the discussion of the magico-religious field in terms of 'force.'

LAURA A. BOHANNAN

**Talking Drums of Africa.** By John F. Carrington. London (Carey Kingsgate), 1949. Pp. 96, 12 illus. Price 5s.

69 As a clear but comprehensive account of an African technique which has been the subject of much romantic nonsense, this book could hardly be bettered. The author has avoided complicated technical descriptions and writes in a popular style which should make the book interesting to a wide public.

The earlier work of Rattray and others has been amplified and developed. Beginning with an account of the language stocks of Africa, he explains the significance of tone in Bantu languages and goes on to show how the tone pattern of words and phrases enables the African to transmit complex messages lasting even up to an hour by means of permutations of the two tones of a talking drum. Words having the same tone pattern are distinguished by the use of conventional phrases of distinctive tone pattern embodying the desired word and usually describing some attribute of the particular word. Thus for the word 'girl' the Kele use the phrase *beseke botlakende linginda*, 'the girl will never go to the fishing net,' fishing with the *linginda* net being a male employment under the traditional division of labour. Many examples and texts are quoted to demonstrate how the drum names given to children at an early age or at initiation are used to specify the identity of the sender and the addressee, as also the name of the village or village section, whilst the opening phrases of a message indicate whether or not immediate action will be required, the type of message following, etc. Chiefs may even 'dictate' on a small drum to their official 'transmitter' using a more powerful instrument. A message is naturally limited to one particular language area, though bilingual drummers can translate and re-transmit messages. Thus the extreme claims of earlier writers as to range of transmission are shown to be inaccurate—especially as five or six miles appears to be the normal range of a talking drum. The role of the drums in announcements of births, marriages, and deaths and such public events as inter-village competitions is fully described.

This technique is extended to other instruments (even to whistling) and the author gives examples of the use of the *sese* in games of hide-and-seek by employing the tone patterns of the drum languages, a practice significantly absent in other areas where the *sese* is found but the drum language is unknown. Comparisons are made with drum-message systems outside Africa, and the distinction is drawn between those based on code, and those, as is usual in Africa, based on tone. The methods of drum-construction are also fully explained.

The author notes that knowledge of the drum languages is decreasing with the growth of new means of communication, and has attempted to foster instruction in this art, but it would appear unlikely that this alone will preserve the art from extinction.

P. M. WORSLEY

**African Music in Northern Rhodesia and Some Other Places.**

By A. M. Jones. *Rhodes-Livingstone Mus. Occ. Pap. No. 4* (N. S.). Lusaka (Government Printer), 1949. Pp. 78, plates. Price 3s.

70 This new edition of the Rev. A. M. Jones's well-known paper has been enlarged by the addition of two articles reprinted from *African Studies* on 'African Drumming' and 'The Study of African Musical Rhythm,' and even if the sections of the book are consequently somewhat disconnected, the author's approach to different aspects of African music gives the book a definite unity. Written in a lively and stimulating manner, it is especially welcome at the present time when its author's radio talks have aroused considerable interest in African music and in his interpretation of it.

Although he covers a large number of topics, ranging from the manufacture of 'Kaffir pianos' to his view that the African scale is probably based on a series of conjunct fourths, the most important part of the book deals with the controversial subject of rhythm in African music. The view that the music should be regarded as 'poly-rhythmic, i.e. made up of voices each carrying their own inherent rhythm, and having different starting points' is polemically argued in a total reconstruction of a West African piece originally noted down by von Hornbostel in which, without altering the original note values or rests, the author makes a completely different analysis, revealing that the complexity of rhythmic interplay is in actuality based on a combination of simple elementary rhythms. Professor Kirby's remark that this article 'appears to . . . mark an epoch' will undoubtedly be echoed by many, even by those critics who may still feel that some limitation of the original title 'African Music' was necessary. Perhaps after this analysis of a West African piece, their number will be fewer.



Worthy of note are the author's insistence on eliminating the subjective element in approaching African music and his assertion that the student must go beyond contemplation to participate in making African music on the spot with the co-operation of the performers, if his analysis is to be correct, as well as his determined attempt to establish accurate standards of mensuration and recording. These are typical of the strictly scientific approach which has enabled him to make such advances in analysis. He acknowledges that they have not been made without 'mental strivings and manual struggles,' but as an example of the value of scientific methodology in the analysis of an art form, his work helps to break down the old dichotomy between 'art' and 'science'—a wider achievement than the immediate analysis of his material.

The practical suggestions for new but inexpensive recording machines (the phonograph is rejected as inadequate for the study of rhythm) and the author's appeal for co-ordination of research procedure must be supported if the study of African music is not to suffer the fate of African linguistics in the past, with a superabundance of individual techniques and an anarchical massing of material, much of which has proved inaccurate or incapable of use for comparative purposes. P. M. WORSLEY

**The Pattern of Hera Kinship.** By J. F. Holleman. Rhodes-Livingstone Papers, No. 17. O.U.P., 1949. Pp. 58, diagrams. Price 6s.

71 This paper is a welcome addition to African literature, for it presents a detailed account of kinship among the Hera, a Shona tribe of Southern Rhodesia. It begins with a section on 'Terminology and Structure' in which kinship terms are defined and certain terms are shown to be associated with lineage groups and generation distinctions. The second section, 'Some Practical Aspects of the Hera Kinship System,' discusses lineage structure, marriage regulations and practices, obligations and behaviour towards kinsmen, and the relation of residence to kin-group membership. Consideration of these matters at such length has, however, meant omission of other kinship material, and our understanding of *mutupo* (totem or clan) and *cidawo* (sub-clan) is little advanced. Some difficulties appear in the text itself. For example, 'tribe,' 'clan,' 'sub-clan,'

'lineage' and 'major lineage segment' are used synonymously; all of these but the last are applied to the Hera. Surely it is time that the customary terminological confusion about the Shona tribes was settled. The terms listed should be applied consistently to territorial, political or kinship groups, and surplus terms abandoned. A plea for some agreement about the spelling of native terms might also be entered. PAULA BROWN

**Handbook on Race Relations in South Africa.** Edited by Ellen Hellmann and Leah Abrahams. Cape Town (London: Cumberlege), 1949. Pp. xii, 778. Price £2 2s.

72 This Handbook is the most authoritative reference source that has yet appeared on the facts surrounding the Union's most crucial social problem. Its many contributors present a mass of detail about the disabilities of the African, Indian and Coloured populations and deal also with such special topics as inter-racial co-operation, race attitudes and the position of the High Commission and Mandated Territories. While it would be naive in the extreme to believe that such a publication would alter illiberal thinking within South Africa, it may do much to inform interested persons and organizations outside that country.

As a contribution to the understanding of the processes underlying race relations, the Handbook reflects vividly certain of the deficiencies of sociological research in South Africa. There is no explicit analysis of the general principles regulating inter-personal relations between Europeans and non-Europeans, nor, probably more important for the political future, between the various non-European groups themselves. The subjects of internal differentiation within these groups, the conceptions they have of each other, and of the psychological consequences of minority-group membership, are hardly touched on.

The Handbook would also have been improved considerably by the inclusion of a summary account of the historical background to the present inter-racial situation, an extended treatment of which already exists in the work of I. D. MacCrone, and of a speculative prognosis. In this latter connexion, it is a sobering thought that, with present trends, the period covered by the Handbook, though implicitly indicted by its contributors, may come to be regarded in the future as part of a relatively rosy past. CYRIL SOFER

## AMERICA

**A Village That Chose Progress: Chan Kom Revisited.** By Robert Redfield. Chicago (Univ. of Chicago Press) (U.K. agents: C.U.P.), 1950. Pp. xiv, 187. Price 21s.

73 This book is related to two of Professor Redfield's earlier works. In *Chan Kom* he gave an ethnological account of this Maya village as it was in 1931. In *The Folk Culture of Yucatan* this material formed part of a stimulating exercise in the theory of social typology. Empirical study and comparison of a city, large town, village, and tribal settlement in a common ecological and cultural setting was, 'in parvo' a study of certain aspects of the historic process of civilization itself.

The Chan Kom material was particularly significant because, in this newly settled village, change was not enjoined by others, but was explicitly desired by the inhabitants. They were led 'to set a goal; to make a program of self-advancement; to define progress in terms of more material wealth, power, comfort, and health; to strive for political and economic power in competition with one's neighbours.'

Such a situation invites examination after a lapse of time: this book is the outcome of a six weeks' visit to Chan Kom after an absence of seventeen years.

Professor Redfield sacrifices the precision and clarity of his earlier ethnological account for a discursive, albeit attractive, presentation. He examines the changes in material culture and considers the partial introduction of a money economy. He notes developments in the division of labour, and describes shifts of emphasis and effect in family and village authority. He recounts the introduction of Protestantism; the reaction to it, in social terms, of Catholic values; and the persistence, alongside both, of the old

shamanistic practices. He discusses contacts with the outside world, and their cultural repercussions within Chan Kom.

*A Village That Chose Progress* presents for criticism no conclusions of theory, nor poses any questions of methodology, as *The Folk Culture of Yucatan* so skilfully did. Instead, we are left, in the final chapter, with a comparison of Chan Kom with American frontier settlements of the early nineteenth century as seen through the eyes of Alexis de Tocqueville. Professor Redfield makes the comparison, and forces us to listen to an overtone of disenchantment with his own culture, as he sums up the people of the Maya village—'a people who have no choice but to go forward with technology, with a declining religious faith and moral conviction, into a dangerous world.' DERRICK J. STENNING

**Notes on Middle-American Archaeology and Ethnology.** Vols. II and III. Vol. II (Nos. 31-60): pp. 257; Vol. III (Nos. 61-90): pp. 244. Carnegie Institution of Washington, 1944-5 and 1946-8

74 In Vol. II of this series (of which I reviewed Vol. I in MAN, 1942, 90 and 1946, 43) H. Berlin writes on a head variant for Eleven, A. V. Kidder and Miss A. O. Shepard on early Guatemalan pottery, and Kidder also on pottery from Copan. J. E. S. Thompson has a paper on inscriptions from Copan, and another on the dating of seven monuments at Piedras Negras, in which he takes the very sound view that towards the end of Cycle 9 the matters in dispute among the Maya scientists appear to have been settled and texts were therefore short, the real period of controversy being at about 9-13-0-0-0. Also he writes on the Altar of Zoomorph O at Quirigua and on variant methods of date-recording in the Jataté area—a very



interesting discussion, though the method of dating can be found to some extent elsewhere. R. L. Roys deals with the Vienna Dictionary—a valuable account of a new MS. My own paper on the Venus Calendar of the Aztec demonstrates the recognition of the Venus period among the Aztec as well as among the Maya. Miss T. Proskouriakoff describes an inscription on a jade probably carved at Piedras Negras; the paper is of exceptional interest because the jade was found in the Sacred Cenote at Chichen Itza, while the stylistic and chronological connexions with Piedras Negras are convincingly worked out. R. H. Barlow writes on the Graphic Style of the Tlaluica. There are two posthumous papers by S. G. Morley, whose death we all regret, one on Glyphs G and F of the Supplementary Series and the other on the Initial and Supplementary Series at Altar de Sacrificios. Lawrence Roys has a very useful set of Moon Tables and Senorita M. A. Espejo has an interesting paper on rock paintings at Texcalpintado, Morelos.

In Vol. III Thompson writes on some uses of tobacco among the Maya, on tattooing, and on the dating of Structure 44 at Yaxchilan. This is a very good paper and helps to unravel some of the puzzles of Yaxchilan. I quite agree that Morley was unduly influenced by his scheme of artistic development, and Thompson's comments thereon are very much to the point. Thompson and Miss Proskouriakoff also write on Maya Calendar Round dates; this also is a very important paper on the question of the position in the months held by the days. R. H. Barlow has several interesting papers, among which we may particularly notice one on the Malinche of Acasingo. He also writes on the Tamiagua Codices, on Mexican figurines of the colonial period, on the Codex of the Derrumbo del Templo Mayor, on geometric ware, on the Tortuga of Coatlan, on stone objects and on the Codex of Tonayan. E. M. Shook deals with blowguns in Guatemala, and with A. V. Kidder he has a paper on rimhead vessels from Kaminaljuyu. R. L. Roys gives a very valuable account of the Book of Chilam Balam of Ixil. F. C. Fulton has a paper on Maya arithmetic and another asking 'Did the Maya have a zero?' I have papers on Maya arithmetic and on observation of the sun among the Ixil of Guatemala. G. Stromsvik and J. M. Longyear III have an interesting paper on El Rincon del Jicque, which is evidently the site described by Galindo, though not necessarily that described by Fuentes y Guzman as the place taken by Chaves. Miss E. McDougall writes on altar sites in the Quiche region and describes the present-day rites celebrated at them.

RICHARD C. E. LONG

**Latin-American Civilization.** By Bailie W. Diffie. Harrisburg, Pa. (Stackpole), 1945. Pp. 812, 36 plates

**75** This well written and extremely interesting book is a comprehensive survey of all the Latin-American countries, with a review of their methods of colonization and their development from the discovery until 1810. It is much too large a work to deal with in detail, even if I were able to cover so wide a field.

The first chapter sets out the purpose of the author to give a change of emphasis in appraising the conquest and colonization, and it is just this change of emphasis with which I cannot agree. Throughout the book it is notable that everything unfavourable to the Indians is stressed, while the cruelty of the conquerors is quietly

minimized. The estimate of the attainments of the Indian culture, as compared with those of the Old World is remarkably accurate, especially that of their progress towards the invention of writings though the Peruvian *quipu* is rather under-estimated. However, the author is always anxious to belittle the Incas. It is quite true that the Indians were not plaster saints, but neither were the Europeans. No opportunity is lost of emphasizing the cruelty, immorality and treachery of the natives, and it is urged that they were before the conquest engaged in an unending struggle for land. But the history of the Old World is exactly similar to that of the New World, as far back as we can trace either of them; there have always been struggles between nations. He enlarges upon the fact that often the Indians helped the conquerors against their fellow aborigines; but has he never heard of quislings and collaborationists? The cruelty of Aztec sacrifices was probably not greater than that of the barbarous public executions of contemporary Europe. Montezuma justified the sacrifices to Cortes on the ground that the victims were criminals or had forfeited their lives by the fortune of war. In sum probably their cruelty was less than that of the ancient Romans with their crucifixions and gladiatorial shows, the latter merely for amusement and not for the safety and welfare of the people, as the Aztec believed their sacrifices to be. And he says nothing about their merciful laws of slavery. He further maintains that the natives merely exchanged one set of masters for another and he exaggerates the power of the Aztec monarchy, although he does not fall into the opposite error, now popular with Americanists, by which these rulers are converted into mere tribal headmen. No doubt the Inca power was much more absolute and no doubt also such a totalitarian socialism could not be maintained without severe penalties, but, making all deductions, the subjects of the Inca were much better treated than their descendants were under European rule. And probably their lot was happier than that of the peasantry of Europe at the period.

It is curious that while the cruelty of the conquerors is minimized, yet elsewhere in the book there are sufficiently terrible pictures of the condition of the natives under Spanish and Portuguese rule. So in fact this portion of the book effectively refutes the thesis of the first chapter.

It is true that, as he says, Latin America, notwithstanding government restrictions, steadily progressed. Such restrictions were the usual practice in those times, and the progress of the Latin-American peoples is due to their own inherent vigour, just as is their continued progress today.

I have not materials to verify all the author's statements and in particular his estimates of the smallness of pre-Columbian population, but it is curious to see Oataheite stated to be in Hawaii. Also he says the Northmen had trading posts from Norway to Vineland; they had indeed colonized Iceland and part of west Greenland but had no trading posts in either country. They only spent three winters in all in Vinland, as their projected colony there failed. They merely landed on the intermediate countries, Helluland and Markland, and seem never to have even spent a night there. In view of these careless statements one wonders if the author's estimates of population are well founded.

RICHARD C. E. LONG

## OCEANIA

**Art in Arnhem Land.** By A. P. Elkin and Catherine and Ronald Berndt. Melbourne and London (Cheshire), 1950. Pp. 123, map, 32 plates (9 in colour). Price 2 guineas

**76** This is an interesting and valuable addition to the literature on Australian aboriginal art. Professor Elkin, who has edited the volume, has contributed the first and last chapters. In these, from his great knowledge of contemporary Australian aboriginal culture, he has summarized the relation of the art to the social system—in particular the symbolic system—of the people; given a brief classification of the main cultural regions of the continent, with their characteristic art forms; and discussed some of the modern problems of maintaining the aboriginal artistic heritage. The body of the book incorporates part of the results of the fieldwork of Mr. and Mrs. Berndt, who spent about three years in Arnhem Land between 1944

and 1947. Among the liberal illustrations the colour plates deserve special mention. A couple of these (the sacred *ranga* emblems in Plate 3A, and the hollow log coffins in Plate 18B) lack clarity; but the majority are very well done. The intricate group of kangaroos and emus, described as a hunting scene and reproduced in yellow, white and red-brown (Plate 14), is delightful. Other coloured reproductions, of carved wooden male and female mythological beings, show a striking unique type of aboriginal art, hitherto undescribed save in a brief paper in *Oceania* (Vol. XVIII, 1948). The suggestion is made that design and manufacture of these figures, which are chipped from the round log, may have been introduced from Indonesia. This is plausible, since wood-carving of this type is found only in this small coastal area of Australia, and as Warner pointed out years ago, in other ways the culture of the Arnhem



Land aborigines has been affected by contact with the 'Malay' traders from Macassar and adjacent areas.

The chapter on Arnhem Land and its people gives a useful though rather tantalizing account of the social structure. The western system is noted as having a tribal organization of corporate linguistic and territorial units with a matrilineal moiety and phratry alignment, and marriage with the daughter of either the mother's brother or the father's sister. The eastern system, of which the Wulamba, termed by Warner the Murngin, are an important part, has patrilineal moieties as a major feature. It is characterized by non-tribal linguistic groups, which operate in the framework of a clan system, each linguistic group being usually constituted by a set of sub-groups, each in a different clan and locality. The clan, which is patrilineal, has conversely in most cases speakers of more than one dialect or linguistic variant among its members. The linguistic group is said to exist as 'an almost entirely independent unit.' This seems doubtful. Considering that the authors list 32 of these 'linguistic groups,' and that membership of a linguistic group is said to be transmitted by patrilineal affiliation, the linguistic usage is obviously not of ordinary domestic kind. Marriage, which is with mother's brother's daughter, is exogamous for clan and linguistic group, so a mother cannot teach her child her own 'language.' The system seems to be one of conventional speech usages of a specific kind, with differences maintained and cultivated to emphasize cross-clan ties. But the situation is not made clear. Its relevance in this connexion is the association of the linguistic groups with totemic art.

The authors bring out in a very elaborate way the social context of the aboriginal art. Many details are given of how the aboriginal artist works. An outline of the three great ritual myths of the Arnhem Land people prefaces a classification of sacred emblems (*rangga*) and a detailed description of the many examples figured. These include conventionalized representations of animals, birds and plants used as totems, and also other symbolic objects referring to events in the myths. Designs painted on the human body, or on bark sheets, are also usually of sacred significance. They represent the clan country and its mythology, and are often shown to men of other groups as a ritual revelation, for which payment must be made. The practice of painting the body is especially developed in north-east Arnhem Land, where complicated patterns are frequent. In western Arnhem Land, apart from bark-paintings of totemic type, or those used to illustrate myths and songs, there is a most striking series of designs used in sorcery. These represent men or women with bulbous limbs and constricted joints, elaborate sex organs, with bird head and stingray spines protruding from all available parts. Figures of this and allied types are regarded as probable developments of the cave-painting art which has flourished

in the Oenpelli-Liverpool-River region. These cave paintings are stated to form one of the most extensive and beautiful series of native drawings known in Australia, and it is a pity that only the bark-painting analogies appear to be represented in this book.

A point of some interest is the way in which alien elements have been incorporated into the aboriginal art. Apart from the wooden human figures mentioned earlier, several items seem to have been adopted from the Indonesian traders. Anchors of their vessels have been imitated in wood, for 'hooking' women in love magic. The vessels themselves have been portrayed in colour on bark sheets, in full sail and equipped with crew, to illustrate songs or nostalgically to recall former days when the visitors came annually from the islands.

In one general matter, the reader is left rather at a loss. Several anthropologists have now published data from Arnhem Land, but they have not managed to maintain effective continuity, by adequate reference to one another's work and by adopting a common system of phonetics. Whereas *mala* is here used for a clan, Thomson (*Economic Structure and the Ceremonial Exchange Cycle in Arnhem Land*, Melbourne, 1949) uses *mälla* for a group of clans. We are told in this book that *j* (italic) has the value of English *y*; but whereas the Bernids use *jiritja* as a moiety name, Warner, Thomson and Elkin use *Yiritja*—and so on. Aboriginal terms for concepts are often hard to identify as between different authors, and clan names used in one book cannot be found in another. From the point of view of primitive craftsmanship and aesthetics, for instance, it would be interesting to know if the Wonggu, a Djapu *dua* man who drew a Macassar *prahu* for the Bernids (Plate 13B) is the same man as Wongo of Caledon Bay, who drew a much better version of the same design for Thomson (*op. cit.*, Plate 6). This criticism is not directed against the present volume as such, but to stress the need for some attention to the collation of Arnhem Land studies.

RAYMOND FIRTH

**Jade of the Maori.** By Elsie Ruff. London (Gemmological Association of Great Britain), 1950. Pp. 89, frontispiece and 14 figs. Price 10s. 6d.

This is a disappointing book. The first 38 pages are largely irrelevant. They provide a survey of the history of stone-working and of the appreciation of jade which is superficial and contains too many unsupported suggestions, such as that the spread of neolithic peoples (including the Polynesians) was largely caused by a search for jade. The fact that jade takes a sharp and tough edge is nowhere sufficiently emphasized. The chapters on chemical composition and Maori nomenclature are useful; that on Maori technical methods might well have been expanded. There is quite a good bibliography and index.

B. A. L. CRANSTONE

## CORRESPONDENCE

**Social Anthropology: Past and Present.** Cf. MAN, 1950, 198, 254 and 271, and 1951, 33, 34, 35 and 62

78 Sir,—With the general argument and conclusions of the Marett Lecture I am in entire agreement. To reconstruct a society from the observations of a single expedition, or of a single generation of observers, is as inadequate as to reconstruct a biological organism from a set of microscope sections. To see either institutions or anatomies intelligently, they must be seen in operation; and this alone inparts a time element, and a study of processes and changes, and therewith a historical method.

The controversy is not peculiar to anthropology. It is unfortunately active elsewhere. In geography, for example, there are attempts to dissociate the study of physical factors from their operations in periods of time; in biology, to ignore or minimize the consideration of processes. But in both these sciences, we are brought back to the experience that in our physical world things are not distributed uniformly, and do not happen uniformly in time. Types of weather, of landscape, of flora, display processes and sequences; their spatial distributions are also historical distributions. And with human societies it is the same. There is indeed no hard and fast line, at which the time dimension ceases to be relevant.

I stress this point, because it is not merely a result of the contrast

between physical and humanist or 'moral' sciences. There is a time aspect in all physical sciences; only in some, and for some purposes, it is possible to discount it hypothetically. Even in astronomy, our information about the stars is ages old, and of different ages: it is only as it reaches us that it is synchronous. So too in any society, some functions and institutions may have remained unaltered for generations or centuries, while others are changing between two visits of the same observer. Only the recognition of these time relations can put the whole composition into harmony. Human institutions are not merely a discarded shell or husk of a society. They *are*, part by part, that society in action, and their function is to carry it out of the past into the future.

I am a little reminded of a saying of Sir Charles Sherrington: 'I would rather trust my motor to my groom, than my horse to my chauffeur.'

Oxford

JOHN L. MYRES

**A Rare Peruvian Figurine.** With a text figure

79 Sir,—The Cambridge University Museum of Archaeology and Ethnology possesses a Peruvian figurine (fig. 1) of most unusual type (museum no. 33.679). It was bought at a sale (at Puttick and Simpson's) in London on 5 October 1933,



with a number of textile fragments from Paracas, to which locality it also was ascribed. Confirmation of this is supplied by a photograph of a very similar example recently figured in *Andean Culture History* (Handbook No. 15 of the American Museum of Natural History), by Bennett and Bird, p. 148, fig. 29, bottom centre.

Such figurines are extremely rare. Apart from the two already mentioned, there was another, which cannot now be traced, in the sale from which the Cambridge example came, since Mr. Louis Clarke, the donor, who was then curator of this museum, wrote on



FIG. 1. A PARACAS FIGURINE AT CAMBRIDGE

Height c.  $3\frac{1}{8}$  inches: C.U.M.A.E. photograph

the museum card, 'Except for the preceding lot (in the sale), I never saw a figurine of the type before.' Dr. L. A. Valcárcel, the well-known Peruvian archaeologist, told me recently that he did not know of any examples in Peru.

The photographs render description unnecessary, except for a few remarks about the colours and material. The figurine is covered with a pale pinkish-buff slip, which is almost greasy to the touch. In one place where this is thin it appears that the ware itself is pale buff. The two stepped bands on the face are picked out in colour, the lower in brick red, and the upper in pale yellow. The band which crosses the face under the eyes and round the nose is brick red. The rest of the face is covered with a thin wash of cream, except for the band containing the eyes and the area round the nostrils and mouth. The incisions were done when the clay was wet.

The pale colours are in marked contrast to the rich, deep ones found on the only examples of Paracas cavernas which I have seen, and it seems possible that the figurine may belong to the later culture of Paracas Necropolis.

G. H. S. BUSHNELL

Cambridge University Museum of Archaeology and Ethnology

#### Polydactyly in Australian Art. Cf. MAN, 1950, 13

**80** SIR.—In connexion with his interesting theory that the engravings at Port Hedland (N.W. Australia) of six-toed feet may illustrate polydactyly as an indication of recessive genes resulting from inbreeding (MAN, 1950, 13), Mr. F. G. G. Rose also refers to the *wandjina* (or *wandjina*) figures painted on rock walls and in rock shelters in the north-west Kimberleys, but misses the very point which seems to support his theory, namely the peculiar bodies and limbs of some—not all—of the *wandjina* figures. There are *wandjina* heads without a body, others with a bean-shaped or kidney-shaped appendix possibly representing a breast ornament. In another category we find crooked figures and also bodies of strikingly small dimensions quite out of proportion with the head. If it were possible to regard these figures as naturalistic pictures with proportions approximately true to life, some of them could be interpreted as representations of pathological cases, and perhaps a few with only rudimentary bodies could be explained as illustrations

of achondroplasia. As far as fingers and toes are concerned, some *wandjina* figures show the normal numbers while others are conspicuous for not only polydactyly but also syndactyly. Professor Elkin tells us that 'in this region the number of fingers represented varies from three to seven' (*Oceania*, Vol. XIX, p. 5). Needless to say, a biological interpretation of the original meaning of these figures would be independent of their function in belief and ritual of the modern aborigines.

However, inaccuracy in the numbers of fingers and toes is common in the graphic art—not only rock art—of the Australian aborigines, especially in the north and north-west. Sometimes fingers and toes, or even hands and feet, are not depicted at all in otherwise well drawn naturalistic human figures, the arms ending at the wrists and the legs at the ankles. Fingers and toes are details, and their omission does not necessarily possess deep significance but merely indicates the 'sketchy' character of the drawing, as I hope to be able to demonstrate elsewhere shortly.<sup>1</sup>

As far as the heads of the *wandjina* figures are concerned, I too am inclined to regard them as naturalistic, but in quite a different sense. As I have explained in *Primitive Art* (Pelican, revised ed., 1949, p. 176), they are undoubtedly skulls, without the mandibles, for not only is a mouth missing but, true to nature, there are neither eyes nor noses, only holes instead. To my great satisfaction Professor Elkin independently arrived at the same conclusion, at least with regard to one, or some, of the *wandjina* (*Oceania*, Vol. XIX, No. 1, p. 12). The conclusion is that any anomalies in the bodies and limbs belonging to *wandjina* skulls are due to the fact that they are representations not of real human individuals but rather of mythical beings, whose imaginary features can hardly be used as evidence for biological theories.

With regard to the Port Hedland engravings, Mr. Rose thinks 'that actual feet with six toes were first outlined and subsequently carved out from the rock, and that the extra digit was not a mistake as is sometimes found in conventionalized drawings of the hand or foot.' As I have not seen those engravings on the spot, the following observation is based solely on Mr. Rose's illustration (Plate B1). I presume, of course, that he has chosen a particularly clear example to illustrate what he regards as a traced 'six-toed' foot. I believe this photograph does not represent the outline of a six-toed, but that of a normal five-toed foot. Tracing the contours of a normal hand or foot will inevitably result in marking the four spaces between the fingers or toes with as many 'notches,' or just strokes. These four strokes together with the two outer contours of the thumb or big toe, and little finger or toe, will always produce a picture as we see it in Plate B1, the toes being indicated by the five spaces framed by the six lines, not by the lines themselves. An illustration of similar engraved tracings of human feet, one with very distinct toes outlined in the same way as the Port Hedland specimen, has been published by Professor Elkin in his article 'The Origin and Interpretation of Petroglyphs in South-East Australia,' *Oceania*, Vol. XX, No. 2, Plate II, D.

LEONHARD ADAM

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#### Note

<sup>1</sup> It may be pertinent to point out that in modern art (as for instance in certain of Henry Moore's reclining figures) the feet often tend to disappear for what seem to be purely aesthetic reasons. There can be no doubt of the functional utility of the human foot in enabling man to remain perpendicular to the surface of the earth, but functionalism is not a passport to success in art; and as soon as the naturalistic canon of classical and Renaissance art is abandoned—together with the civilized convention, virtually absent from tribal art, of drawing in the ground as a line or a plane under the feet of the figures—the foot not only becomes less necessary, but begins to present to the painter or sculptor problems of composition which may on occasion be most artistically solved by its omission.—ED.

#### Correction: MAN, 1951, 11

**81** In the January issue, the price of Wilhelm Gerloff's *Die Entstehung der öffentlichen Finanzwirtschaft* should have been given as DM. 3.50, not DM. 350.









(a) Type A hut, Doringberg, O.F.S.



(b) Type B hut, Sedan, O.F.S.



(c) Type B hut, Doringberg, O.F.S.



(d) Baphuthi store hut, Tsoloane, Basutoland



(e) Ama Ntande grain store, Quthing, Basutoland



(f) Batsoeneng fire hut, Thoahlane, Basutoland



(g) Batsoeneng fire hut, Thoahlane, Basutoland

# CORBELLED STONE HUTS IN SOUTHERN AFRICA

Photographs by James Walton



# CORBELLED STONE HUTS IN SOUTHERN AFRICA\*

by

JAMES WALTON

**82** Scattered throughout the eastern part of the Orange Free State, between the Orange and Vaal Rivers, and in southern and eastern Transvaal are extensive early settlements consisting of circular stone kraals and corbelled beehive huts. Brief accounts of some of these have already been published<sup>1</sup> and I am at present engaged in making a more detailed survey. They belong to two main groups. One, found along the Walsch, Rhenoster, Wilge and other Vaal tributary valleys, is characterized by circular kraals and huts which stand free. In the other group, which is distributed over the more eastern areas, the huts and kraals are usually linked together and have common walls. The resemblances are sufficient to indicate that both had a common origin, but that at some period a split occurred and subsequent development followed rather different lines.

The corbelled huts of the Orange Free State and Transvaal fall into three distinct types which most probably represent an evolutionary sequence. The simplest, type A, are true beehives, usually built from boulders of spheroidal dolerite (Plate D*a*) or irregular blocks of sandstone which curve inwards on all sides until an opening from 12 to 18 inches in diameter is left at the top. This is closed by either a single flat stone slab (fig. 1, *vi, a*) or by four flat slabs with a fifth over the opening between them (fig. 1, *vi, b*). The doorway is low, usually not more than 2 feet high, as was common in all types of early Bantu dwelling when it was necessary to keep out wild animals. It is usually about 18 inches wide at the base, widening slightly towards the top. The lintel is a single large block of stone or two or three long flat slabs (fig. 1, *iv*). These huts have an external diameter of between 9 and 11 feet and an internal diameter of from 5 to 7 feet, which agrees with Scotch and Irish examples and seems to be the maximum diameter which corbelling in boulders would allow. The height is rarely more than 4 feet. The walls have a thickness of about 2 feet 6 inches at the base, decreasing to about 1 foot at the top. A large slab of stone is often found near the entrance and was apparently used to close it during the daytime when the owners were away.

Huts of this type are often incorporated into the kraal wall and at Doringberg, near Ventersburg, and at Sedan, near Lindley, in the Orange Free State, the entrance is frequently protected by a circular or semi-circular stone wall enclosing a paved courtyard, or *lelapa*, having an internal diameter of from 6 to 8 feet (fig. 1, *i*).

Adjoining these simple beehive huts at Sedan and Doringberg, although not actually associated with them, are corbelled huts of a different type, type B (fig. 1, *iii*). These are certainly later in date and, judging by the number of querns, they were built by a people who were pastoralists and also cultivators of kaffir corn, whereas apparently little agriculture was practised by the occupants of the earlier dwellings.

\* With Plate D and three text figures

The type B hut (Plate D*c*) evolved in response to a desire for increased living room. The internal diameter was often as much as 9 feet and the height up to 6 feet. The doorway varies in height from 2 feet to 3 feet 6 inches and the walls are often built of flatter slabs of dolerite in place of the spheroidal boulders. This choice did not depend on the material available, for both types of rock occur in the same region. It indicates rather an improved building technique. The walls rise, with only a slight inward curve, to a height of from 3 feet 6 inches to 4 feet and then curve inwards

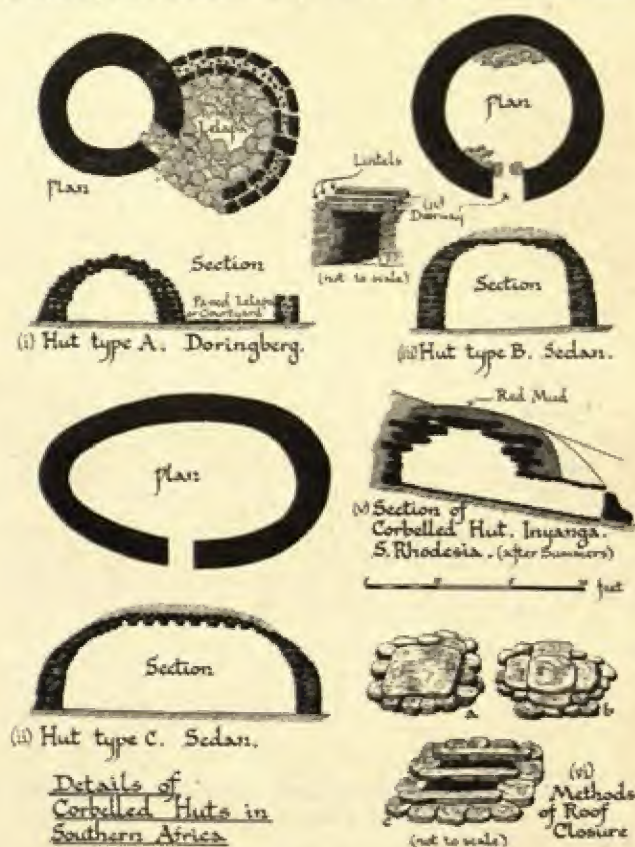


FIG. 1

sharply as far as the corbelling would allow, which was about 2 feet. The central opening was then closed by three or more long slabs of dolerite, set about 4 inches apart (fig. 1, *vi, c*), and covered with rubble and broken sherds to give the roof a flattened dome shape which rendered it watertight (Plate D*b*). Even today, when much of the rubble has been removed, these huts are still rainproof and I have often sheltered in them during heavy rain. They invariably stand free; they are never joined together or incorporated into the kraal wall; nor have they a *lelapa* in front of the entrance.

The corbelling technique apparently did not permit of building a circular hut with an internal diameter of more



than 9 feet. Even this could only be achieved by the use of long slabs to cover the opening at the top and, as these were not obtainable in lengths exceeding 3 feet, the size was limited. In order to obtain more space within the hut the only possibility was to extend the hut along one axis to produce an oval shape, type C. There is a group of huts of this type at Doringberg whose internal diameters vary from 8 feet by 10 feet to 8 feet by 11 feet 6 inches. The roof is covered by a series of long slabs stretching from front to back on which stones and rubble were piled. In all other respects these huts are identical with type B. At Sedan a single hut of this type, which is egg-shaped in plan, has an internal length of 16 feet and would comfortably house six or more adults (fig. 1, ii).

The pottery, iron spearheads, querns and human remains associated with these stone huts indicate that they are of Bantu origin, and the Hoernlés and Van Riet Lowe have suggested that the settlements at Tafelkop and Vechtkop were built by the Leghoya or Bataung, the earliest Bantu invaders from the north, whilst Van Hoepen has ascribed the Lydenburg remains to the Bapedi.

According to tradition the Leghoya were the first to cross the Vaal and settle along the Wilge, Rhenoster, Zand and Walsch river valleys and they were also reputed to be the best builders in stone of all the early Bantu. They occupied this area from 1650 to 1810 when they were conquered and completely absorbed by the Bataung under Moletsane. After troublesome times from 1824 onwards the Bataung and the remnants of the Leghoya finally settled along the southern border of Basutoland, where they still live. Their houses, whether in mud or stone, are still superior to those of the other Sotho tribes.<sup>2</sup>

Enquiries among these Bataung revealed that up to about ten years ago their normal dwelling was a corbelled stone hut, known as a *sefala*, almost identical with the type B huts already described. Although many Bataung were met who had seen, built and occupied such huts I have not been able to find a single complete example. A number of walls still stand but the corbelled roof has been replaced by a conical thatched roof.

All my informants agreed, however, that the *sefala* had a cylindrical wall, up to 14 feet in diameter, which rose vertically to a height of about 5 feet and then curved inwards until an opening remained which could be closed by a single flat stone. The outside was often smeared with mud. The entrance of later examples was about 4 feet in height but the older people remember when the doorways were so low that the occupants had to crawl in. The entrance was closed by a flat stone with a hole in the centre through which a finger could be inserted to pull the stone in place from the inside of the hut. From three to five people occupied a single hut. The fact that the Leghoya and Bataung lived in the tributary valleys of the Upper Vaal and that they continued to build corbelled huts until quite recently is proof that they were the builders of the now deserted settlements in that area. Their own traditions support this.

The Leghoya were not the only builders of stone beehive huts and the early settlements cannot all be ascribed to

them. Almost at the same time as they crossed the Vaal River, about 1650, the Batsoeneng and Baphuthi entered Basutoland from the north-east and ultimately settled along the Caledon River and along the Orange Valley.<sup>3</sup> The Batsoeneng, in the region of Qeme and Kolo, still build corbelled huts, also known as *lifala* or *libopi* (Plate Df, g). They are about 8 feet in diameter and from 5 to 7 feet high. The walls, unlike the thick walls of the Leghoya huts, are only a single course in thickness and they curve inwards only slightly until a height of 4 or 5 feet is reached, when the central opening is covered with a flat stone slab. The doorway is from 2 feet 6 inches to 4 feet in height although formerly it was much smaller. The hut is plastered with clay and cow dung both inside and out and is normally used today as a store hut or fire hut, although in days gone by it was the traditional dwelling of the Batsoeneng. When it is employed as a fire hut the opening at the top



FIG. 2. BATSOENENG FIRE HUT IN SECTION, AT THOAHLANE, BASUTOLAND

is usually covered by a mat of stout reeds which thus affords an exit for the smoke. In rare instances the walls curve evenly from the base to the apex and there is no opening at the top (fig. 2).

Almost identical structures, with small lintelled doorway openings, are used by the Baphuthi for storing grain or as poultry huts (Plate Dd) and I have also found them in use as grain stores by the Ama Ntande (Xhosa) and the Ma Vundle in the Mjanyane Valley of South Basutoland (Plate De). The Ma Vundle are a branch of the Bafokeng who crossed the Drakensberg into the Transkei early in the seventeenth century and during the following two centuries were almost completely absorbed by the Xhosa. Whether their corbelled grain store is a survival of an early Bafokeng dwelling type or was acquired from the Xhosa it is difficult to determine. They assert that the corbelled grain store (*selulu*) was formerly commonly employed by the Xhosa peoples. Its form certainly indicates that it originated as a dwelling.



Van Riet Lowe has suggested that the corbelled stone huts in the Transvaal and Orange Free State were built by the first Bantu peoples to move into this area and that they turned to stone for their huts because of the absence of trees.<sup>4</sup> Although both corbelled stone and mud beehive huts no doubt had their origin in the wattle beehive, yet the wide distribution of the corbelled hut among a number of now totally disconnected Bantu tribes does not support the theory. The distribution pattern of the corbelling technique in Southern Africa (fig. 3) indicates that it was brought from the north and had reached south-eastern

entered by a tunnel, 1 foot wide and 2 feet high which could be closed by means of a stone slab. Summers believes that others exist in the same area (fig. 1, v).<sup>5</sup>

All the available evidence indicates that the corbelled beehive stone hut in Africa originated somewhere to the north of Inyanga, that it moved southwards along the highland ridge to south-eastern Transvaal towards the beginning of the seventeenth century and from there spread into the Upper Vaal tributary valleys, into Basutoland and into the Transkei. How far to the north of Inyanga was the source of origin we cannot yet tell, as I

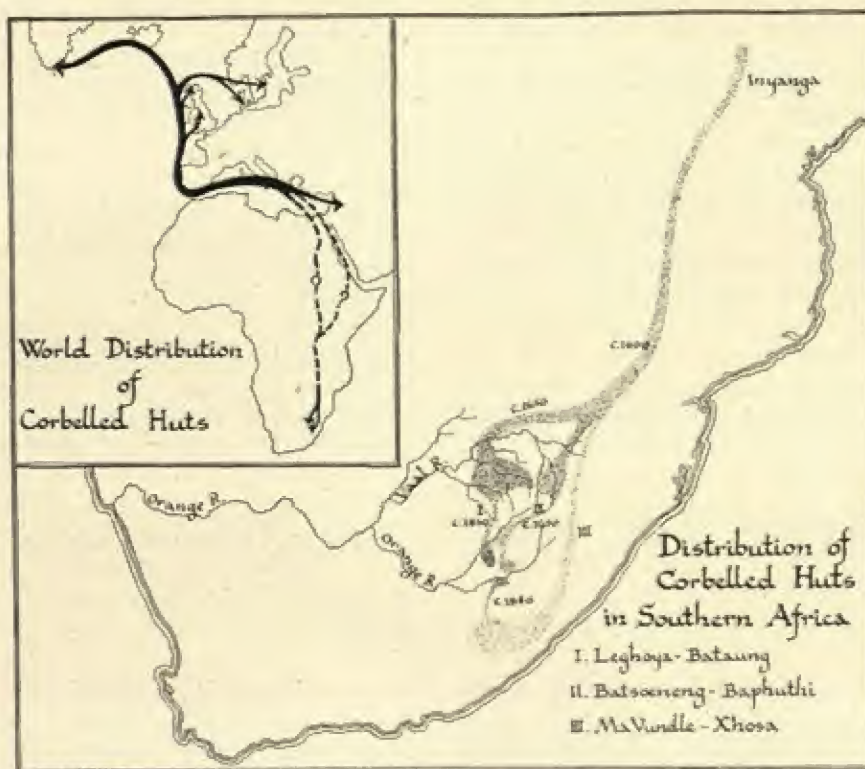


FIG. 3. DISTRIBUTION OF CORBELLED HUTS

Transvaal by the beginning of the seventeenth century. There a split occurred and its subsequent dispersion followed three main routes: that of the Leghoya into the Upper Vaal, that of the Batsoeneng and Baphuthi into south-west and south Basutoland and that of the Xhosa into the Transkei. It is extremely unlikely that each of these three groups invented the very difficult corbelling technique independently and much more probable that it was the traditional house type of the first Bantu peoples to reach southern Transvaal. After the split each group evolved independently, giving rise to the distinct Leghoya-Bataung and Batsoeneng-Baphuthi-Xhosa types.

Further support for this theory is afforded by the recent discovery by Roger Summers of a corbelled beehive hut in the Inyanga district of Southern Rhodesia. This hut has an internal diameter of 8 feet and is constructed of large rounded boulders covered with a coating of red mud; it is dug out of the hillside and has a sloping paved floor. It is

am not aware of any corbelled huts having been recorded in those areas. World distribution of corbelled huts (fig. 3) indicates that they originated in the Mediterranean, probably in the late Neolithic or early Bronze Age, somewhere in the vicinity of southern Italy where corbelled stone huts, *trulli*, have persisted to the present day.<sup>6</sup> From there the style stretches continuously westwards through Sardinia, the Balearic Islands, Spain and Portugal<sup>7</sup> to Western England,<sup>8</sup> Wales<sup>9</sup> and Ireland<sup>10</sup> and so by way of the Scottish Isles<sup>11</sup> to Greenland<sup>12</sup> and Scandinavia.<sup>13</sup> The continuity of this distribution precludes any possibility of independent evolution, especially when one considers the complexity of the technique. Eastwards the style spread to Greece, and southwards, either by way of Ethiopia or down the Rift Valley, to Rhodesia and so into the Transvaal and the Orange Free State. Until stone beehive huts are discovered in Ethiopia or the Sudan this can be nothing more than a hypothesis, but the distribution



of corbelling certainly follows two well defined routes focusing on the Mediterranean in the neighbourhood of South Italy and Greece where it would appear to have originated.<sup>14</sup>

There is a striking similarity between the evolutionary development of the corbelled stone huts in Western Britain and those in Southern Africa. The earliest examples in each case are true beehives of rough boulders partly excavated out of the hillside. Later examples, as typified by the *clochans* of Fahan, County Kerry, and Inishmore, County Galway, are identical with types B and C in South Africa. The same technical limits were imposed in each case and the solution was the same. First the walls approximated more and more to a cylinder, e.g. the *clochans* on Blasket Island, and then the oval form developed. The oval *Clochán na Carraighe*, Inishmore Island, County Galway, is roofed by means of a row of large parallel slabs extending from front to back to cover the space between the two long sides of the hut exactly like the oval hut at Sedan. In areas so widely separated the desire for increased living room, controlled by the limitations of the corbelling technique, resulted in an identical development.

#### References

- <sup>1</sup> (a) C. Van Riet Lowe, 'A Preliminary Report on the Stone Huts of Vechtkop,' *J. Roy. Anthropol. Inst.*, Vol. LVII (1927); (b) Mr. and Mrs. R. F. A. Hoernlé, 'The Stone-hut Settlement on Tafelkop,

near Bethal,' *Bantu Studies*, Vol. IV (1930), pp. 33-46; (c) E. C. N. Van Hoepen, *A Pre-European Bantu Culture in the Lydenburg District*, Nat. Mus. Publ., Bloemfontein, 1939.

<sup>2</sup> D. F. Ellenberger, *History of the Basuto*, 1912, pp. 165-9.

<sup>3</sup> Ellenberger, *op. cit.*, pp. 21-30.

<sup>4</sup> Van Riet Lowe, 'Pre-European Stone Structures in South Africa,' *S. Afr. Archit. Record*, August, 1944, p. 207.

<sup>5</sup> R. H. Summers, letter to me dated 22 September, 1950. I am greatly indebted to Mr. Summers for this information. A more detailed account of this corbelled hut will be published by him in the projected monograph on Inyanga.

<sup>6</sup> Lancelot G. Bark, 'Bee-hive Dwellings of Apulia,' *Antiquity*, Vol. VI, 1932, pp. 407-10; (b) Paul Wilstach, 'The Stone Beehive Homes of the Italian Heel,' *Nat. Geog. Mag.*, 1930, pp. 228-60.

<sup>7</sup> P. Bosch-Gimpera: *Etnología de la Península Ibérica*, 1932, pp. 490ff.

<sup>8</sup> James Walton, *Homesteads of the Yorkshire Dales*, 1947, p. 8.

<sup>9</sup> Iorwerth C. Peate, *The Welsh House*, 1944, pp. 42-5.

<sup>10</sup> Ake Campbell, 'Notes on the Irish House,' *Folklore*, 1938, pp. 173-83.

<sup>11</sup> F. L. W. Thomas, 'Notice of Beehive Houses in Harris and Lewis,' *Proc. Soc. Antiq. Scot.*, Vol. III (1857-60), pp. 127-44; *id.*, 'On the Primitive Dwellings and Hypogea of the Outer Hebrides,' *Proc. Soc. Antiq. Scot.*, Vol. VII (1866-8), pp. 153-93.

<sup>12</sup> T. C. Lethbridge, *Merlin's Island*, 1948, pp. 94-7.

<sup>13</sup> Campbell, *op. cit.*, p. 174.

<sup>14</sup> Evidence of early migrations from the Mediterranean down the Nile Valley and from Ethiopia into Southern Africa is afforded by a number of recent papers, e.g. (a) Henri Breuil, 'Some Foreigners in the Frescoes on Rocks in Southern Africa,' *S.A. Archaeol. Soc. Bull.*, Vol. IV (1949), pp. 39-50; (b) S. Davis, 'A Coin of Pisa,' *S.A. Archaeol. Soc. Bull.*, Vol. V (1950), pp. 55-9; (c) G. A. Wainwright, 'The Founders of the Zimbabwe Civilization,' *MAN*, 1949, 50.

## STONE AGE MAN'S USE OF POWER\*

by

B. A. L. CRANSTONE, M.A.

**83** The palaeolithic stage of culture is normally correlated with a hunting and collecting economy. Except by magical means man does nothing to increase or regulate his food supply, and his control of his environment is therefore very limited. Dwellers in tropical forests, such as the Vedda, move round their tribal territory on a regular yearly route governed by the movements of game, the ripening seasons of fruits or roots, or the availability of wild honey. The Australian aborigines similarly follow the seasonal variations of their food supply. The Eskimo are static during the winter and follow the caribou or other game during the summer; and Upper Palaeolithic man in Europe seems to have followed a similar routine. Only a few such peoples had a sufficiently sure food supply to enable them to settle permanently in one place: such were the Fuegians and some of the Mesolithic tribes of Europe, who lived largely on shellfish.

People living this kind of existence have neither the resources to make machinery nor the need for it; in any case it would be an impossible encumbrance. When the food supply is widely scattered, any application of power

which is to be useful must be light enough to be carried by a man. The only source of power which is portable and always available is the muscular power of the individual; and Grahame Clark<sup>1</sup> has calculated that even if the muscular power of such a wandering group could be concentrated to one end it would not generally exceed one horse power.

This power of the individual can however be augmented or concentrated in various ways. The main opportunities present themselves in the field of missile weapons. The spear-thrower adds in effect an extra joint to a man's arm and thus increases the leverage applied in his throw. The bow stores power during the draw and releases it instantaneously; the same is true of the multitude of traps depending on the elasticity of bent saplings. The blowgun concentrates the power of a man's lungs on the small area of the pith or cotton wadding at the base of the dart. Thus the bow concentrates power in time, the blowgun in space.

There may also be scope for some ingenuity in the preparation of foods. The Australian aborigines prepare *nardoo* on flat stones with a stone as rubber. These stones are highly valued, but are too heavy to be carried on the periodic wanderings, and in any case are only useful in the restricted localities where *nardoo* grows. They are therefore

\* The substance of a paper read to Section H of the British Association at Birmingham on 4 September, 1950, as the opening contribution to a discussion of 'Man's Use of Energy.'



hidden near the camping site when the time comes to move on. The North American Indians and other peoples used hollows in boulders in which to pound seeds or nuts. Upper Palaeolithic man in Europe used pestles and mortars, though possibly only for preparing pigments.

Such handicrafts as the manufacture of personal ornaments are not usually highly developed in a palaeolithic culture; but there may, as among the Eskimo, be a place for the drill, with a stone, bone or shell point. The arrow-straightener is an application, possibly the first, of the lever principle. It is certainly used by the Eskimo, but it is less certain whether the similar object made by Upper Palaeolithic man in Europe was used for the same purpose. Skins, which among hunters are plentiful, are used if clothing is required, and the loom is therefore unnecessary. Cord is a necessity, unless it is replaced by hide thongs, but it can be made simply by rolling fibres on the thigh. Food surpluses are only such as can be eaten in a few days, and preserved food is too bulky to be carried; moreover most of the foods available are not easily preserved. So there is no need for containers other than skin bags, gourds or other natural objects.

In general most of the so-called 'simple machines'—lever, wedge, pulley, screw, inclined plane and wheel—are not appreciated or needed at the palaeolithic level of culture. The Eskimo, it is true, understood the principle of the lever and applied it in the case of the arrow-straightener and the oar, but their culture is much richer than those of the typical hunters and collectors. The digging stick, used by the Bushmen, the Australian aborigines and many other peoples, also applies the principle of the lever, but it remains true, as a generalization, that hunting and collecting man made little progress in harnessing power except in the field of weapons.

Some hunters and collectors knew the grinding technique: many Australian tribes used edge-ground axes, and the Andaman Islanders used shell adzes made by a grinding process.<sup>2</sup> But in the neolithic stage man has usually discovered or adopted the practices of agriculture and domestication of animals. As he partly or wholly produces his own food he now needs a much smaller area for his sustenance and can settle in one place, though owing to poor agricultural technique he may exhaust the soil quickly and have to move every few years.

Except among nomadic pastoralists many of the factors limiting man's use of power now no longer operate. Large settled communities grow up, in place of the small usually migratory groups of hunters. It is no longer necessary for the machine to be portable: it could be large and static, and could be erected at the source of power. Wind and water power thus for the first time become theoretically available. An agricultural life provides a surplus above immediate needs. This must be stored for use between harvests and in bad years, and a whole new series of needs in the field of containers and buildings is realized. Part of the surplus can be used to support non-productive members of the community, who are set free to develop political, religious or aesthetic ideas and to improve the technical

equipment. Moreover the change in diet from mainly meat to mainly vegetable means that animal skins for clothing are no longer plentiful, except where animal husbandry predominates. Alternative materials have to be found, and the loom now appears.

The domestication of animals, which apparently took place at about the same time as the beginning of agriculture, provided a potential source of power for static machinery, a means of transport, and when applied to the plough made possible a great increase in the productivity of the cultivated land. As a source of power for static machinery animal power was however not used until about the fifth century B.C.<sup>3</sup>; the animal-drawn plough was apparently not invented, in Egypt, until the early Dynastic period,<sup>4</sup> when copper tools had been in use for some time.<sup>5</sup> Pack animals were probably used early, but wheeled transport was a later invention.

Lilley has pointed out that a means of transport is of some importance to the progress of milling machinery.<sup>6</sup> During the Neolithic period in Europe and western Asia—before the development of advanced agricultural techniques, of irrigation machinery or of the elaborate political system necessary for the organization of large-scale irrigation schemes—settlement was relatively sparse. It follows that milling machinery could not be an economically sound proposition unless transport was available to bring the grain from a wide area.

Apart from animals, the other possible natural sources of power seem not to have been used in this period. The first known instance of the harnessing of water power, the water wheel, dates only to about 100 B.C., though the wheel and the chain of pots for raising water occurred in the Near Eastern Bronze Age.<sup>7</sup> The use of wind power seems to be as old as the harnessing of animals, but only in the form of the sailing boat; other applications of wind power were very much later.

The needs and opportunities of modern primitive peoples in the tropics, still or until recently using ground stone tools and practising agriculture or horticulture, are somewhat different. The root crops, yam and taro for instance, are mashed or cooked whole, and do not require grinding machinery, though a pounding device might be useful. The same is true of sago and cassava, though in different ways the preparation of both is complicated. The Maya ground their maize on the *metate*, a form of saddle quern; and this highly civilized people never invented a more efficient machine for this purpose. In fact the most ingenious method of food-preparation indigenous to America is probably the cassava-squeezer of the Amazon region. This is a plaited cylinder with a loop at each end. One loop is fastened to a house beam or something firm, the other part-way along a pole, the end of which is flexibly attached to a firm object, forming a lever. By exerting pressure on the lever the cylinder is stretched and therefore constricted, squeezing out the poisonous liquid.

Thus peoples in the neolithic stage of culture seem to have made relatively little progress in the harnessing of power, in spite of their great achievements in other fields.



At least the grain-eaters among them could have made use of machinery in the preparation of food, yet they did not even invent the rotary quern. Some of them could with advantage have used irrigation machinery. They had concentrations of population sufficient to make milling machinery economically feasible. They had leisure between crops, and the surplus of food which seems to be necessary for the attainment of higher culture. In many cases they had a potential source of animal power. Yet they produced nothing more complicated than the cassava-squeezer, the saddle quern, the sail (not applied to static machinery) and various simple types of weaving apparatus.

The reason cannot be that it is impossible to make machinery with stone or shell tools; one need only mention the Polynesian canoes, or Maori or Trobriand wood carving, to show that both robust and precise work is possible. The general failure of stone-age peoples to make and use machinery is clearly not due to a lack of manual skill. The reason must lie partly in a lack of appreciation of opportunities; perhaps also in lack of external stimuli, such as growing pressure of population or climatic changes, like that which probably conduced to the adoption of a settled life when desiccation set in over the fertile areas of Egypt and south-west Asia. But I suggest that the lack of metal tools was an important factor, for the following reasons.

Primitive man will spend hours, if he can spare them from hunting, collecting or growing his food, in making articles of ritual, ornamental or prestige value; but he will not give the time and labour to making a machine unless the eventual result is a saving of both. The essence of nearly every machine is that it has moving parts or bearings, on which the wear is concentrated. Generally speaking the only material of which primitive machinery can be made is wood: and wood moving on wood wears out quickly. The loom is an exception in that the wear is evenly distributed; and we find it widely used among stone-age peoples, for instance in America, Micronesia and parts of Melanesia. The drill, in any of the forms used by primitive peoples, involves little wear of wooden parts, for the point can be made of a harder substance; it was used almost universally in America and widely in Oceania. But water-raising or food-preparing machinery, and of course the wheel and axle, almost inevitably involve wood moving

on wood or stone. With only stone tools the frequent replacement of worn parts must absorb much of the time and labour saved by the machine, and so reduce its economic value. It therefore seems likely that the development of most sorts of machinery was not practicable until metal tools were not only known but generally available.

The sequence of events in the Near Eastern civilizations is interesting in this context. Copper tools were in general use in Egypt by the time the pyramids were built. No machinery more complicated than the lever, sledge, roller and ramp, with the wedge for quarrying, seems to have been used in their construction.<sup>8</sup> However the replacement of barges, rollers and wooden apparatus in general would have occupied a far larger proportion of the total man-hours if copper tools had not been available. There is no evidence of irrigation machinery in Egypt or the Asiatic countries before the copper age, though it may have existed<sup>9</sup>; and it seems that the wheeled vehicle did not appear in Asia until after the discovery of the smelting process, though at some sites, for example Tepe Gawra, models of carts are found from a period when stone tools were still used for some purposes.<sup>10</sup> In this connexion it may be significant that although the wheel was not used for any practical purpose in Pre-Columbian America wheeled toys have been found at a number of different sites in Mexico.<sup>11</sup> It seems that although the principle was known its possible applications had not been appreciated, or perhaps it could not be applied with economic efficiency.

I wish to express to Mr. Adrian Digby my gratitude for a number of suggestions and references.

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- <sup>1</sup> Grahame Clark, *From Savagery to Civilization* (1946), p. 30.
- <sup>2</sup> A. R. Radcliffe-Brown, *The Andaman Islanders* (1933), p. 447.
- <sup>3</sup> S. Lilley, *Men, Machines and History* (1948), p. 24.
- <sup>4</sup> Clark, *op. cit.*, p. 90.
- <sup>5</sup> A. Lucas, *Ancient Egyptian Materials and Industries* (1948), pp. 228f.; I. E. S. Edwards, *The Pyramids of Egypt* (1947), p. 212.
- <sup>6</sup> Lilley, *op. cit.*, p. 41.
- <sup>7</sup> Lilley, *op. cit.*, p. 32.
- <sup>8</sup> Edwards, *op. cit.*, Ch. VII *passim*.
- <sup>9</sup> Mr. C. J. Gadd (in conversation).
- <sup>10</sup> V. G. Childe, *What Happened in History* (1942), p. 66.
- <sup>11</sup> *Cuadernos Americanos*, I (1946); D. Charnay, *The Ancient Cities of the New World* (1887), fig. p. 175.

## OBITUARY

### Alexander Low, 1868-1950

**84** Emeritus Regius Professor Alexander Low, M.A., M.D., LL.D., of the Chair of Anatomy in the University of Aberdeen, died on 15 November, 1950, in his eighty-third year. He entered the University in Arts in 1886 and after taking his M.A. graduated in Medicine. He became a member of the staff of the Anatomy Department in 1894 and Professor in 1925. Although he retired in 1938 his connexion with the University and its students was maintained upon various committees until the last. He was a past President of the Anatomical Society of Great Britain and Ireland, of the Aberdeen Medico-Chirurgical Society, a Director of the Aberdeen Royal Infirmary, a Governor of the Royal Mental Hospital and a member of the Scottish

Universities Entrance Board. He represented the University on the Board of the Rowett Institute for Research in Animal Nutrition.

He had an international reputation for his research upon the development of the lower jaw, and Aberdeen University Anthropological Museum, thanks to his enthusiasm, possesses probably the finest collection of the skeletal remains of the Short Stone Cist or Beaker people. The care, precision and detail with which he conducted his investigations upon their burials, both at the site of discovery and in the laboratory, were admirable. He was a connoisseur in the appreciation of a skull. He would often delight to show fellow anthropologists a skull exactly typical of the Bronze Age in every respect and then point to the disarming



evidence of a dental filling. As a physical anthropologist at a meeting in Oslo some fifteen years ago, he was being teased by cultural anthropologists that he could come to no definite decision upon the racial origins of his companions. 'What can you tell us,' they said, 'of the ancestry of the young woman who is conducting our bus tour at the moment?' 'Well,' said Professor Low, 'one of her parents might be Norse and the other an Aberdonian.' There was no answer to the derision evoked but to question the girl herself. 'My father,' she said, 'was Norse and my great-grandmother came from Aberdeen.'

He made careful anthropological records for many long years. *The Annals of Eugenics* for October, 1950, publishes his article upon 450 male and 450 female infants measured at birth. Still to be published are his records upon 66 male and 60 female children each measured annually from birth to five years, approximately 160 measurements for each one of the 126 children; these measurements are unique in that they refer to the individual child followed for five years—six periods of measurements. A further series dealt with twins and triplets measured annually and followed in certain cases for a period of eighteen years. As a rule measurements of this kind at present available do not refer to the individual child followed annually over a period of years, although it is difficult to determine this point from the publications.

He knew his students well from many aspects and he went to a great deal of trouble to assist boys who were in difficulty, financially or through illness, and accordingly he was very affectionately known to many generations of students as 'Daddy Low.' Upon the occasion of his retirement his students and friends presented him with his portrait etching by Malcolm Osborne and a cheque for some £500, which has been left for the benefit of medical students.

One of a family of ten children, he is survived by three brothers and three sisters, to whom we extend our deep sympathy.

R. D. LOCKHART

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- 1930-1931. 'Report on Human Remains found in Short Cists in Angus and Nairnshire,' *ibid.*, Vol. LXV.
- 1932-1933. 'Report on Human Remains found in Short Cists in Roxburgh and Sutherland,' 'Two Short Cists at Upper Boyndlie, Tyrie, Aberdeenshire,' and 'Report on Human Remains from Two Bronze Age Cists at Sprouston, Roxburghshire,' *ibid.*, Vol. LXVII.
- 1933-1934. 'A Short Cist at Kirkhill, Kiltarlity, Inverness-shire,' and 'Description of the Human Remains from a Long, Stalled Chambered Cairn, near Midhowe, Rousay, Orkney,' *ibid.*, Vol. LXVIII.
- 1934-1935. 'Report on Human Remains from a Long, Stalled Cairn, The Knowe of Yarso, Rousay, Orkney,' 'Beaker from a Cist at Mill Farm, Rathen, near Fraserburgh, Aberdeenshire,' and 'Cist Cremation Interment at Ury, Stonelaven, Kincardineshire,' *ibid.*, Vol. LXIX.
- 1935-1936. 'A Short Cist containing a Beaker and other relics at Newlands, Oyne, Aberdeenshire,' *ibid.*, Vol. LXX.

## REVIEWS

### GENERAL

**An Introduction to Social Anthropology, Vol. 1.** By Ralph Piddington. Edinburgh (Oliver & Boyd), 1950. Pp. xxvi, 442, 6 plates. Price 25s.

85 It has been a complaint among some teachers of anthropology in Britain that while they disagreed with American anthropologists their students had no other textbooks than American ones. Dr. Piddington has tried in this book to give a first comprehensive introduction to British social anthropology for 'beginners, particularly University undergraduates.' It consists of chapters on: The Science of Primitive Culture; A Cook's Tour of Primitive Peoples—Africa and America, Asia and Oceania; Social Organization; The Principles of Cultural Analysis; Food and Wealth; Land Tenure; Primitive Law; Religion and Magic; and The Native and Ourselves; with appendices consisting of an ethnographic directory, a kinship exercise, and primitive communism ('an experiment in semantics'). There are twenty-one figures and four maps. Vol. II is to be published later, and will deal with applied anthropology, the common methods of psychology and anthropology, the applications of anthropological methods to the study of modern communities, culture contact, etc.

Any pioneer attempt such as this is bound to meet with objections from other anthropologists, but this one is perhaps open to more than one would expect. One concerns the plan of the book. The two 'Cook's Tour' chapters seem unnecessary and do not justify the space they occupy. A textbook of social anthropology cannot hope to be at the same time a handbook of ethnography, and there is no reason why it should when there are such books as *Our Primitive Contemporaries* and *Habitat, Economy and Society*. The

author's two ethnographic chapters contain 'little scientifically organized information' and consist mainly of 'ethnographic gossip' (p. 31), which seems an unprofitable way of using seventy-five pages. In themselves they are unsatisfactory: there are seven pages for the whole of Africa and the same number on the Eskimo, and ten for the whole of America; the section on Malaysia is absurdly short (two pages) and devoted mainly to the Semang, while the area is the only one illustrated by no map. The section on the Karadjeri (whom the author himself has studied) is a disproportionate twenty-eight pages, and its interest does not make up for only twenty lines on the whole of Polynesia. The book would have been a better one if the author had used the space for more adequate discussion of social institutions or of the history of social anthropology: this latter is hardly touched upon, and then misleadingly: 'The science of anthropology had its beginnings in the latter part of the nineteenth century, when scholars, under the evolutionary influence of Darwin, began to speculate about the meaning and history of social institutions' (p. 11). For the beginner in particular the lack of any general discussion of the anthropological problems and solutions of the past, and of the refinement of questions and observation by which social anthropology has become what it is now, is a serious omission.

Controversial or objectionable points are many: some are inaccurate, some merely confusing, some inadequate. For example, while the term 'ethnology' is better avoided, 'ethnological' is recommended as convenient (p. 3); 'spiritual' or 'social culture' is hardly current usage (p. 4); a dubious rejection of 'blood relationship' in favour of 'consanguineous' is made on the grounds that



many primitive peoples do not think of kinship in biological terms and because the former rules out adoption (p. 111); for the distinguishing of classificatory kin by description, naming, looking, pointing, and tonal inflection the author substitutes pretentious 'indices of identification' (circumlocution, nominal, ocular, manual, and tonal) (p. 124); an age grade is apparently also called an age set or age class (p. 176); the presentation of initiation is scanty and makes no mention of Van Gennep (p. 178); nine pages are given to the discussion of stratified societies, but only one, illustrated by no society, to segmented societies (p. 199); while negative sanctions frequently rest on magico-religious beliefs, it seems that we know these beliefs to be unfounded (p. 326); and discussion of, for example, incest, joking relationships, and sanctions seems unnecessarily attenuated and superficial.

It would be easy to make many more criticisms of the author's definitions, phrasing, emphasis, and presentation; but by far the most controversial point is his conception of the aims and method of social anthropology. He is frankly pragmatic, and disapproves of the distinction between pure and applied science: 'The aim of any science is to study a specified part of the real world and from a study of facts to formulate theories which shall serve as recipes for human conduct,' which he softens by adding 'whether that conduct shall be the carrying-out of further research or the taking of practical steps for the promotion of human welfare' (p. 7). Social anthropology is repeatedly (almost *ad nauseam*) referred to throughout the book as a science; but the beginner is told nothing of the important differences of opinion about this conception, and is given little idea what sort of recipes for conduct such a science has given or ought to be able to give. There is a hint when he says that 'an understanding of the laws governing human behaviour and human relations is essential to the proper application of the resources of science to the promotion of human welfare' (p. 9).

The subject matter of social anthropology is 'culture' (p. 398); and the 'real task of scientific social anthropology,' according to Dr. Piddington, is the study of the dynamic functions of items of culture in relation to the needs of any given community and of the complex interrelationships existing between them (p. 219). It is the author's opinion that 'the postulate of human needs, whether overtly stated as such or not, is implicit in all satisfactory cultural analyses; that it is a theoretical concept necessary to bring social anthropology into relationship with the other social sciences, particularly psychology; and finally that it is absolutely essential to the constructive contributions which the anthropologist should make to human welfare' (p. ix). 'The postulate of human needs enables us to consider our own institutions, customs and values in terms of an ultimate normative conception of how far they contribute to human happiness or misery' (p. 403). Apparently, too, 'some scientifically elaborated system of values is indispensable, and the theory of needs provides such a system' (p. ix).

Now a review is no place for a discussion of the 'theory of needs' and 'culture'; but the intelligent beginner is bound to feel puzzled by the unspecified promise of the 'theory' and the almost hypothesized nature of the pseudo-entity 'culture,' as they are presented to him in this book. He learns that the culture of a people is 'the sum total of the material and intellectual equipment whereby they satisfy their biological and social needs and adapt themselves to their environment' (p. 3); he looks on the map for the location of 'cultures' instead of societies (pp. 35, 72); he is told that culture is an 'adaptive mechanism,' that beliefs are instilled and reinforced by 'powerful cultural forces' (p. 356), that 'any culture necessarily postulates, not only that its standards of action ensure life, but also that obedience to them ensures a successful life' (p. 383); and he is invited to attempt a cultural analysis, to 'try to discern how far the various features of Eskimo culture—including local organization, material culture, magic and religion—are related to the needs for protection against climate, ventilation and the securing of an adequate food supply' (p. xi). But he will be a little dissatisfied, perhaps when he asks 'why, since all human cultures are founded upon a set of universal human needs, they should vary from each other' (p. 244), and gets the answer that 'we can demonstrate that [cultural developments] are related to certain integrative needs in the societies concerned, but we cannot say why these needs have led to

the particular cultural responses which we observe.' Nor does it seem likely that he will be more satisfied by the two undefined 'ancillary hypotheses' with which the theory is reinforced: 'cultural efflorescence' ('We therefore affirm that such striking and exceptional developments . . . have been due to a process of cultural efflorescence, although we are unable to describe this process in detail'), and 'cultural degeneration.'

Whatever the merits or demerits of these concepts, practically or philosophically, it does seem a pity that Dr. Piddington could not have presented the beginner with a more neutral body of theory as held and taught by British social anthropologists, instead of Malinowskianism.

But, theoretical bias aside, this is a useful book and one for which the beginner will be grateful: it gives elementary definitions and illustrations of most of the terms of social anthropology, emphasizing their importance and making reference easier by printing them in bold type, a most helpful device; other less common or more specialized terms are similarly emphasized in italics; instruction is given in the elementary steps of making and understanding kinship diagrams, while hypothetical English genealogies make easily understandable principles of descent, inheritance, residence, etc., found in other societies; and it covers, though briefly, the main topics of social anthropology. Extremely useful are the bibliographical commentaries which follow each chapter: they indicate sources of information and recommend and evaluate other, sometimes more specialized, works on the subjects of the chapters (in the beginning, for example, the student is advised to read *Human Types* as the best introduction to this *Introduction*); while, to modify the theoretical partisanship, the reader is told which authors and theories are opposed to, or differ from, the author's views.

If this is not the textbook we have been waiting for, it will nevertheless be read by all students beginning the study of social anthropology, for in many ways it gives them help that no other book has yet done. If it does not satisfy other anthropologists they are free, with Dr. Piddington's work as an example and guide, to undertake themselves such an arduously ungrateful labour.

RODNEY NEEDHAM

**Race and Culture.** By Robert Ezra Park. Glencoe, Ill. (Free Press), 1950. Pp. xxii, 403. Price \$5

**86** The author, who died in 1944, spent his youth as a journalist and teacher in various parts of the world, and most of the rest of his life as Professor of Sociology at the University of Chicago and at Fisk University, where he seems to have been a most successful teacher. His admirers have collected, and published in book form, a number of his review articles, lectures and introductions to books, written at various times between 1913 and 1942, and nearly all dealing with relations between the Whites and Negroes, or natives and immigrants, in the U.S.A.

All the articles, etc., are well written and readable, and show the author to have been a man of wide reading and wide sympathies, and the book may serve as a useful introduction to the subject of race relations. There is, however, little in it which will be unfamiliar to those who have made any study of this subject.

RAGLAN

**Magic: A Sociological Study.** By Hutton Webster. Stanford Univ. Press, California (London: Cumberlege), 1948. Pp. xv, 524. Price \$6

**87** This is a lengthy discussion of magical procedures among primitive peoples. The author's method of analysis has been to devote a chapter in turn to each of the various elements into which magical rites can be separated: rites, spells, charms, taboos, qualities required in magicians, professional status, publicity or privacy of performance, moral evaluations, degree of faith or scepticism, and so forth. He has then collected together under each heading the relevant material from a number of books about primitive peoples in various parts of the world. The result is a useful general textbook on primitive magic which may be recommended to students who want a synopsis. It has gained in value by the author having taken many of his illustrations of the magic art from the most recent intensive studies of primitive peoples by professional anthropologists.



It would have gained more had he given us a critical assessment of his sources instead of assuming that the authorities he quotes all have equal weight. The author does not reach any original theoretical conclusions, being content to follow older writers such as Marett, King, Crawley, Carverth Read, and others, particularly Sir James Frazer. He agrees with Frazer that magic is best regarded as pseudo-science. He concludes his account thus: 'Considered in the large, magical beliefs and practices have operated to discourage intellectual acquisitiveness, to nourish vain hopes that can never be realized, and to substitute unreal for real achievement in the natural world. Between the methods of magic and the methods of science how impressive the contrast! The choice of one or the other has long confronted humanity.' E. E. EVANS-PRITCHARD

**White Settlers and Native Peoples.** By A. Grenfell Price. Melbourne (Georgian House) (U.K. Agents: C.U.P.), 1950. Pp. 232. Price 25s.

88 The author, who is Master of St. Mark's College in the University of Adelaide, has made an extensive study of the past relations between the native races of the U.S.A., Canada, Australia and New Zealand, and, by personal inspection, of their present conditions. The relations in all four countries fall into three phases, which can be summarized as first massacre and expropriation, second doles and disease, and third more or less enlightened attempts at rehabilitation.

In what are now the United States of America the British Government made attempts to protect the Indians from the settlers, but after the Declaration of Independence these were abandoned, and for a hundred years the settlers were allowed to massacre and expropriate the Indians with impunity. The second phase developed about the end of the last century, and since President Roosevelt's New Day for the Indians dawned in 1932, well organized and largely successful efforts have been made, to rehabilitate the Indians and encourage them to resume their native cultures.

On the whole the Indians of Canada have been treated far better

than those of the U.S.A. The policy has been, and still is, to educate them and absorb them into the European population.

The Australian record is a black one, largely owing to the primitive character of the Blacks and the bad character of many of the early settlers. In recent years the state governments have done a good deal for the Blacks, but the attitude of the Federal Government is still far from enlightened.

The early settlers in New Zealand treated the Maoris very badly, encouraged them to massacre one another, and employed every species of chicanery to deprive them of their lands. During the last seventy years, largely owing to the work of able half-bred Maoris, they have rehabilitated themselves, and now form an increasingly important part of the population.

The book is full of interesting, though mostly unpleasant, facts, but the large number of figures, of populations, sums allotted for various purposes, etc., makes it rather difficult to read. The author habitually misuses the word 'decimate.' The book is illustrated with many photographs, mostly of houses and schools built by the various governments for the native peoples. RAGLAN

**The Beginnings of Diplomacy: A Sociological Study of Inter-tribal and International Relations.** By Ragnar Næmlelin. London (O.U.P.) and Copenhagen (Munksgaard), 1950. Pp. 372, bibliog. Price D.Cr. 40.

89 This is a vast array of statements about various aspects of inter-tribal and international relationships, collected assiduously but uncritically from historical and ethnographical writings. RAGLAN

**Mirror for Man: The Relation of Anthropology to Modern Life.** By Clyde Kluckhohn. London (Harrap), 1950. Pp. 288. Price 12s. 6d.

90 This is the very welcome English edition of a book of which the American edition, reviewed by Sir John Myres in MAN, 1949, 103, has been in frequent demand by users of the Royal Anthropological Institute's Library. WILLIAM FAGG

## CORRESPONDENCE

**Differential Exchange Rates.** Cf. MAN, 1951, 11

91 SIR.—Classic examples of Treasury gains from variable weights and measures are afforded by Chinese history. Thus, before the Republic, when the monetary unit of account was the infinitely varying *tael*, or ounce of silver (of no less variable fineness), and the unit of currency the copper 'cash,' the process of recording each payment of taxes involved an exchange transaction over which the subject might lose heavily and the collector gain an even larger 'dash' than the Ashanti court officials or the king of Dahomey. Morse (*Trade and Admin. of China*, 1921 ed., p. 96) cites a land-tax note on which 'Treasury taels were converted into cash at 2,600 and converted back at 1,105, whereby a tax of Tls. 70.66 was converted into a payment of Tls. 166.20.' Viewed dispassionately, such a case was not an individual abuse, but the norm of an administrative and fiscal system produced by complex social and historical factors. D. J. DUNCANSON

London

**Research in Anthropometry**

92 SIR.—The Medical Research Council have appointed a Committee on Growth and Form, under the Chairmanship of Professor P. B. Medawar, F.R.S., with the following terms of reference:

'To advise and assist the Medical Research Council (a) in promoting research on the growth and form of the human body with particular regard to the provision of basic scientific data, including the elucidation of factors affecting the rate of growth of the whole body and of its parts and the establishment of norms of stature and weight for different ages and different sections of the population; and (b) in facilitating the application of such data to various practical problems, including such as arise within the field of clinical medicine.'

One of the Committee's first tasks is to make a review of work in the field of anthropometry. The Committee would accordingly be glad to have information about current or completed anthropometric surveys, including unpublished records, so as to avoid any unnecessary repetition of work and to ensure, if possible, that use is made of existing material. It is believed that there are in existence records of height, weight and other body measurements which, if analysed, could form the basis for research into growth and development.

Serial records of the growth of the same children from year to year, as well as measurements which have been recorded over a long period of time, would be particularly valuable.

M.R.C. Committee on Growth and Form,  
The Medical School, Hospitals Centre,  
Birmingham, 15

E. M. B. CLEMENTS  
Secretary

**Race.** Cf. MAN, 1950, 220, and 1951, 28-32

93 SIR.—I read with considerable interest the comments on the U.N.E.S.C.O. statement on race which appeared in the January issue of MAN. Since I played a small part in formulating the U.N.E.S.C.O. statement, I would like the opportunity to make reply, not in the spirit of defence or cavil, to certain propositions advanced by Professors Vallois and Hill.

It would be entirely possible but not particularly fruitful to reply in detail to the several criticisms which Professors Vallois and Hill have submitted. It seems clear to me, however, that these criticisms stem from much broader issues, i.e. from different conceptions of the field and status of physical anthropology, from different conceptions of the problem of racial classification and its relevance for understanding human behaviour.

It is further evident to me that Professors Vallois and Hill speak



from the tradition of classical physical anthropology. For example, almost all of Professor Hill's comments are based on the gratuitous assumption that 'races,' in fact, do exist; and that they exist as rigorously defined, genetically homogeneous, and discrete entities. One needs only a little ingenuity, therefore, and the proper scientific attitude to 'discover' where, in Nature's 'booming and buzzing' confusion, they are hidden. This particular form of methodological naïveté has, of course, long bedevilled the history of racial classification and analysis. Professor Hill's comments only serve to beg the entire question: his fears about 'racial interbreeding,' his conviction that valid correlations exist between racial membership and 'intelligence,' indicate that he has not seen fit to ask whether, in any scientific sense, there are any such groups (races) at all. It has already been pointed out that the anthropologists' incessant concentration on group differences has been an important factor in the widespread fixation on the biological boundaries of different groups.

I prefer to ignore Professor Hill's captious admission that he doesn't know what 'racial discrimination' means; instead, I pass on to his specious claim that 'temperamental and other mental differences are well known to be correlated with physical differences.' The only possible sense in which these alleged correlations 'are well known' is that they have played such a great part in the popular folklore and superstition of the western world; they have also been the chief stock-in-trade of the racist ideology and outlook—cf. Gobineau, Houston Stewart Chamberlain, Madison Grant, Sir Arthur Keith and R. R. Gates. Professor Hill's reference to 'the well-known musical attributes of the Negroids,' compares very favourably with the standard folk myths about the American Negro and his so-called 'inborn' traits, e.g. 'superior sensitivity to jazz rhythms,' 'greater sexual capacities,' 'child-like nature,' 'less sensitivity to pain,' and so on.

Moreover, Professors Vallois and Hill do not appear to be aware of the obvious difficulties in attempting to correlate physical differences with mental differences. First, and I think they would agree, race as a classificatory and analytical concept is highly suspect. Second, there may be some small comfort in the fact that although the concept 'race' is of dubious scientific quality, the concepts 'temperament,' 'intelligence' and 'mental differences' are, if anything (as a glance through the literature of the behaviour sciences will confirm), practically useless as research guides or correlates. Third, with few exceptions, all propositions based on claims that behaviour is significantly correlated with physical type leave unchallenged the sociological explanation of human behavioural differences and similarities, i.e. the socio-cultural conditioning and structuring of human needs, motivations, rewards and behaviour patterns. It is hardly good scientific procedure to erect a theoretical edifice which purports to explain human behavioural similarities and differences on biological grounds, yet which leaves an adequate and competing theoretical framework completely untouched. Few inquiries into this problem make clear the known insignificance of 'race' differences for understanding human capacity and behaviour.

I must confess that I do not see the relevance of Professor Hill's query: why interfere (with nature's grand design—the evolution of different racial types)? To my knowledge, the U.N.E.S.C.O. statement does not purport to 'interfere' with anything (excepting, perhaps, Professor Hill's convictions about 'race'). His query, however, can only be based on a belief that 'racial antagonisms' have a biological base and that they are a part of the evolutionary processes; that 'interracial' strife is nature's crude but effective way of fitting 'each (race) to its own environment.' The credibility of this mystique is in no way improved by citing the works of Sir Arthur Keith on these matters. Keith's pronouncements on race have never enjoyed widespread acceptance in scientific circles nor have they been granted the status of scientifically demonstrable propositions.

And now to matters of a somewhat different nature. Advances in human genetics have served to expand the scope of social biology, a problem focus which seeks to articulate and systematize the manifold interrelations of human biological and behavioural factors. These advances, as we know, are the result of a shift in biological analysis from taxonomic-descriptive studies to studies of function, process and diversity. It is to be expected, therefore, that some of

the problem interests of classical physical anthropology will give way to problems of a different intellectual structure. This shift in emphasis and conceptualization has already begun in the United States (and elsewhere). The *Yearbook of Physical Anthropology* (1947) reports that there has been a steady decline in the publication of descriptive racial studies, studies of 'race mixture,' constitutional typing and anthropometry. And few of these followed the classical tradition, most being oriented about human genetics and biology. Similarly, university courses in 'Race Relations,' 'Race Differences' and 'Interracial Conflicts' are rapidly dropping out of university and departmental curricula. Courses in physical anthropology, at many major American universities, have been entirely re-structured; many no longer bear that name. Intellectual responsibility demands that one recognizes the absurdity of attempting to analyse problems which involve demonstrable differences in learned behaviour through the erroneous and untenable framework of 'race.'

I feel certain that Professors Vallois and Hill are not unaware of these and other developments in contemporary anthropology and human biology. For their protestations are, in part, a reaction against the fact that the scientific frontier has pushed beyond the cephalic index, the bigonial diameter, the bizygomatic diameter, and all the rest. But more importantly, I should think it clear that when only the physical attributes of a human organism are known, there is nothing that can be predicted about the behaviour of that organism; only when a social context is specified for the organism does the predictive value of such attributes increase.

I would like to add that I do not subscribe to certain propositions contained in the U.N.E.S.C.O. statement. I share with Dr. Little his conviction that there is no evidence 'for any scientific belief that man is born with biological drives towards universal brotherhood and co-operation.' I am also not convinced that substituting 'ethnic group' for 'race' makes for any appreciable clarity. I do support, however, in substance and principle, the remaining conclusions contained in that statement. These comments and observations are mine alone; I do not speak for the members of the panel or for the members of the revising committee.

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DON J. HAGER

#### Note

Classical (or, as they should perhaps now be called, Old World) physical anthropologists may be permitted to wonder whether the present state of physical anthropology in America as a whole really presents as startling a problem in social anthropology as our correspondent's letter would suggest. There may well be sound political reasons in some countries for the adoption of shibboleths, but on the scientific plane readers of MAN will no doubt hope to discover in the course of this correspondence whether American physical anthropologists generally regard 'intergroup,' for example, as synonymous with 'racial,' and if so whether they do not feel that they are defining themselves out of existence.

It is to be hoped that the difficulty in distinguishing between Houston Stewart Chamberlain and Sir Arthur Keith is not widespread.—Ed.

**94** SIR.—In so far as anthropologists make use of biological terms, the term 'race' is apt to confuse, for in zoology 'race' means a distinct group of animals, descending from a definite couple of ancestors. In this way one may speak of a 'pure race,' when there is no cross-breeding.

A so-called human race is only phenotypically defined, and to speak of a 'pure' human race is practically nonsense. So I agree with Professor M. F. Ashley Montagu that in anthropology it must be recommended to avoid the word 'race,' which can be replaced by the words 'division' and 'sub-division.' The use of the phrase 'ethnic group' in physical anthropology must be rejected.

Furthermore in my opinion there is no reason to deny the fact of these divisions and group differences. They are a large enrichment of human features and abilities, contributed by nature and enabling people to cope with environmental difficulties better and fitting them for various trades.



What we need in the first place is that students should be taught the meaning and the handling of these terms. And if a U.N.E.S.C.O. Statement could contribute to this desirable result, the Organization would certainly deserve our thanks.

A. DE FROE  
Department of Anthropology, University of Amsterdam

**95** *Sra.*—The etymology of the word 'race' is important but obscure. Krogman<sup>1</sup> thinks that 'it may be akin to Czech *raz*, referring to artery or blood, or to Latin *generatio* or old French *generace*.' For Trevor<sup>2</sup> 'the most likely suggestion is that it comes from *ratio*, signifying "mode," "quality," "nature," in which sense it was used by such classical Roman authors as Varro, Cicero and Caesar.' Fowler<sup>3</sup>, on the other hand, in condemning 'racial' as a 'questionable adjective' and a 'recent regrettable formation' which he includes in 'an ill-favoured list' of hybrid derivatives, states that 'The fault in "racial" is not that the Latin word is made unrecognizable by the spelling, but that there is no Latin word from which *race* is known to come.'

My own belief is that it entered the major European languages, as so many other words have done, from Arabic sources. Some Arab writers used *rās* (chief, head, origin, beginning) in ways that might easily have suggested the word *raza* (Spanish) for a kinship group, or followers of a *rās*. The Portuguese *raça* and Italian *razza* could have been derived either directly or from the Spaniards.

A further linguistic token of genealogical piety amongst the Arabs, which is a primitive form of 'racial' thinking, is the term *mowallad* (from which *mulato*, *mulâtre* and *mulatto* are derived) for the offspring of Arab fathers and non-Arab mothers, as well as 'cultural hybrids.' Ibn Khaldun<sup>4</sup> uses it in both senses.

But whatever the etymon of 'race' may be—and it is a point on which philological authorities could profitably be consulted—the essential content of the word, in all its equivalents and meanings, implies the biological transmission of innate qualities. Even its usage in the apparently different sense of a contest, which is a test of certain inbred qualities, does not change its basic meaning.

This unity of meaning is not, of course, peculiar to 'race.' All words expressing relationships have essential meanings that remain intact no matter how differently they seem to be employed. Thus, the notion of succession inherent in 'time,' or of discrimination in 'tact,' remains the same whether we use the former as a measure of change or of music, and the latter as expressive of social sensitivity or as a term for a musical beat.

The same persistence of thought content is evident, too, in apparent synonyms arising from different symbolism. For example, as Cassirer<sup>5</sup> has said, 'the Greek and Latin terms for the moon, although they refer to the same object, do not express the same intention or concept. The Greek term (*μήνη*) denotes the function of the moon to "measure" time; the Latin term (*luna*, *luc-na*) denotes the moon's lucidity or brightness. Thus, we have obviously isolated and focused attention on two very different features of the object.'

It follows that (a) the only essential meaning of 'race' is an unalterable biological one; (b) the nature of this meaning gives the word a highly charged and suggestive content valuable to patriots and politicians. Consequently, it is difficult to understand how Little<sup>6</sup> would 'instruct the public in the proper anthropological use and meaning of "race" as a group concept,' since neither the public nor anthropologists have, or can have, any other meaning for the word than that embedded in it. For the same reason such expressions as 'biological race' and 'sociological race' are logically inadmissible. Words for clear concepts are not qualified in scientific methodology, while incompatible propositions (sociological race) cannot be accepted within the same body of knowledge<sup>7</sup>. Indeed, in the precisely controlled language of an axiomatic system a statement (race) and its negation (sociological) would be impossible<sup>8</sup>.

Methodology offers, I feel, a further comment on the use of 'race' in the rule that a falsified concept or proposition can no longer be maintained. And the concept of human races has been so extensively falsified since *We Europeans*<sup>9</sup> that an increasing number of social scientists are being driven to qualify and explain their usage of 'race' or, more sensibly, to avoid it altogether. This process is superbly illustrated by some forty papers on intergroup problems in the *American Journal of Sociology*, 1938-48. In fact, Little, whose

note arises partly from a confusion of 'ethnic group' with 'race,' is more than a trifle late in his devotion to a discredited word.

Nevertheless, it may be said that there remains the question of using 'race' in accordance with its meaning and without distorting qualifications. This would depend, I believe, on an authoritative and widely accepted classification of human types. And, as a zoologist, I have been so often entertained by the taxonomy of anthropologists that it seems necessary to emphasize the dependence of such a classification on the tenth edition of the *Systema Naturae* of Linnaeus, the rules of the International Commission on Zoological Nomenclature<sup>10</sup>, and long experience in the systematics of fossil and living vertebrates. In the last resort, however, taxonomic decisions are so much matters of opinion, represented at the extremes by the 'splitters' and the 'lumpers,' that the most reputable statement on the classification of mankind would not necessarily 'command the assent of all recognized authorities'<sup>11</sup>, unless those serious students who might have different views are to be outlawed by Act of U.N.E.S.C.O.

The results of a methodologically acceptable classification of the *Hominidae* would show that its living representatives belong (a) to one species in which infra-specific categories cannot be differentiated; or (b) that subspecies of *Homo sapiens* can be clearly recognized and named according to the Law of Priority and not the fancies of name-makers; or (c) that *Homo sapiens* is really a complex of distinguishable species, in which case each would probably have identifiable subspecies as well.

The first result would leave anthropologists free to construct a typology of human groups according to their evidence and experience, but avoiding the terms of taxonomic zoology and accepting the invalidation of the race concept with reference to living men. The second or third results should deprive them of the pleasures of typology, since the difficult oddments would be grouped tentatively as forms (a useful 'neutral' word in biology) of the various definite categories.

It is clear, I think, that none of these three results would encourage the scientific use of the word 'race,' though subspecies can be colloquially regarded as races. I say colloquially because subspecies is the only infra-specific category acknowledged by the International Commission on Zoological Nomenclature. Some taxonomists do, of course, name variable forms of subspecies as varieties, thus converting the trinomial system into an officially ignored quadrinomial one, but they expose themselves to the sarcasm of sterner colleagues. The occasional practice of recording laboratory strains, 'habitat varieties' and other lesser categories as races, for which Trevor<sup>12</sup> has expressed 'sympathy,' is a tiresome aberration contrary to the serial arrangement upon which the universal intelligibility of zoological nomenclature depends. The enthusiastic individualist is a menace to all classificatory systems.

In conclusion, it would appear that those who do not believe in race, but use the word 'sociologically' as a concession to supposedly popular currency, must always suffer the inconveniences of a brittle logic. The believers, on the other hand, who use 'race' in a manner consistent with its meaning, must await or provide a confirmatory taxonomy, which at best would do no more for the use of the word than justify it as a zoologically obsolete synonym for subspecies. And both schools should realize that the quaint anthropometric and opinionative indulgences of 'racial' anthropology, despite their value as incentives to academic humour<sup>13</sup>, have no place in the *Science of Man*.

CEDRIC DOVER

#### References

- 1 W. M. Krogman in R. Linton (ed.), *The Science of Man in the World Crisis*, New York (Columbia Univ. Press), 1945, p. 38.
- 2 J. C. Trevor, s.v. 'Race,' *Chambers's Encyclopaedia*, London (Newnes), 1950, Vol. XI, p. 428.
- 3 H. W. Fowler, *A Dictionary of Modern English Usage*, Oxford (Clarendon Press), 1926.
- 4 M. G. De Slane, 'Les Prolégomènes d'Ibn Khaldoun, Part 3,' *Notices et Extraits des Manuscrits de la Bibliothèque Impériale* (Paris), Vol. XXI (1868), pp. 300 and 344 (footnotes).
- 5 E. Cassirer, *Essay on Man*, New Haven (Yale Univ. Press), 1944, p. 134.



- \* K. L. Little, 'U.N.E.S.C.O. on Race,' *MAN*, 1951, 31.  
 † F. Kaufmann *Methodology of the Social Sciences*, New York, 1944.  
 ‡ J. H. Woodger, *The Axiomatic Method in Biology*, C.U.P., 1937.  
 § J. S. Huxley and A. C. Haddon, *We Europeans*, London (Cape), 1935.  
 || W. T. Calman, *The Classification of Animals*, London (Methuen's Biological Monographs), 1949.  
 ¶ Editorial Note, *MAN*, 1951, 32.  
 \*\* J. C. Trevor, 'The Physical Characters of the Sandawe,' *J. Roy. Anthropol. Inst.*, Vol. LXXVII (1950), p. 63 (footnote).  
 †† J. Barzun, *Race—A Study in Modern Superstition*, New York (Harcourt, Brace), 1937, and London, 1938.

## Note

Mr. Dover's letter has been shown to Dr. Trevor, who makes the following reply:

96 SIR,—It is comforting to know that Mr. Cedric Dover, who wrote so forcibly and to such purpose as a younger man, remains quixotic enough to try and deliver Dr. Little and me, together with certain of our fellow anthropologists, from the tyranny of words, or of one in particular, to which, like a second literary knight, we are devotedly in thrall. At the same time, I find it hard to see why Mr. Dover fails to understand Dr. Little's suggestion that the public should be instructed in the anthropologist's concept of race, and then goes on to tell us that this concept has been widely 'falsified' since *We Europeans*, where, I believe, 'ethnic group' was first substituted for 'race.' Perhaps an inclusive 'since' is what he has in mind; or, to paraphrase Dr. Little, it may be that 'anthropologist' is as diabolical a term as 'race' or 'native' (because anthropologists study both races and natives) and should be exorcized, if Mr. Dover will forgive the academic humour, by the process of calling a spade a geotome. I have no brief to speak for Dr. Little, but it is clear that he was gravely at fault in not realizing that the contributors of some forty papers on 'intergroup' problems to the *American Journal of Sociology*, 1938-48, had discredited 'race' and 'racial,' and he must take the consequences. As Mr. Dover is still worried about confusions, he might turn to a small popular work by Professor Vallois, who very succinctly distinguishes 'race' from, if I dare anglicize what I feel would be a useful addition to our vocabulary, 'ethny,' and both from 'nation.'<sup>1</sup>

Mr. Dover says that 'race' has 'no classical ancestry,'<sup>2</sup> and in view of the title of the book in which that statement appears, I was rash indeed to attempt to meet his, and Fowler's, objection. My authority for deriving *razza* from *ratio* is Casaliini, who thinks that *rationem*, the accusative form, would give rise to *ragione*, employed by Boccaccio and Machiavelli for 'species, kind and nature.'<sup>3</sup> The Czech *rdz* and the O.F. *generace* are rejected by Oberhummer, who with Mr. Dover, and Dozy, Baist, Kluge and Pianigiani before him, prefers the Arabic *ras*.<sup>4</sup> The earliest use of 'race' in English known to me occurs in line 50, 'And bakkyttaris of sindry racis,' of Dunbar's poem *The dance of the sevin deidly symnis*, probably written during February, 1508, new style.<sup>5</sup> While I am open to conviction about the origin of 'race,' I venture to suspect Mr. Dover's etymology of 'mulatto.' This comes, surely, by way of Portuguese or Spanish, from *mulus*, and on account of a belief also held by such an 'inspired and inventive racialist' as Paul Broca.<sup>6</sup> I beg to differ, too, from Mr. Dover when he asserts that 'race' as a contest or trial of speed has the same 'essential content' as 'race' in its biological or classificatory sense. He could, I fancy, trace the ancestry of the first through the O.E. *res*, running, to the Sanskrit *rish*, to flow; but 'basic meanings' are best left to semasiologists, among whom he may doubtfully and I certainly cannot be numbered.

Mr. Dover's experience of being diverted by the taxonomic efforts of anthropologists is shared by many sterner colleagues in their own field, to judge from the frequent discussions of the topic in the literature.<sup>7</sup> If Mr. Dover is familiar with these, he doubtless considers them ineffectual and his gentle didacticism regarding the tenth edition of Linnaeus, the law of priority and other parts of the *Règles* and so on to be therefore all the more necessary. It will, I imagine, be a relief to him to discover that the excellent little monograph by Dr. Calman to which he refers was recently noticed in *MAN*<sup>8</sup>

and could even have encouraged its Hon. Editor's fond hope, abhorrent in the eyes of Mr. Dover, that anthropologists and others might agree on a statement embodying established facts about race.<sup>9</sup> Perhaps not all the faculty would concur with Mr. Dover's notion of the results of a 'methodologically acceptable' classification of the Hominidae, and I wonder myself whether some of his ideas are not methodologically a bit awry. Be that as it may, the Don in Mr. Dover really tilts at windmills, I feel, in his comments on a footnote to a quaint anthropometric indulgence of mine where I blush to confess that I have followed techniques initiated in Britain by the 'racist scribe' Galton.<sup>10</sup> In my footnote, I cite, approvingly, a concept of race among fishes advocated in a paper by no less an enthusiastic individualist than Professor Hubbs, and I suppose that it might apply also to man. What a pity that Mr. Dover did not bestir himself sufficiently to consult that paper (or otherwise he would seem to be quite as perverse as I am), for in it Hubbs states that he envisages race as a distinct, if minor, systematic category and not as a synonym of subspecies; that a quadrinomial designation for such races would be both impracticable and contrary to the International Rules; and that such races are not included in the current code of zoological nomenclature because, among other reasons, they 'are ordinarily distinguishable only by average characters that may call for statistical treatment' and so 'the routine identification of single specimens or small samples would often be difficult or impossible.'<sup>11</sup>

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J. C. TREVOR

## Notes

- <sup>1</sup> Henri-V. Vallois, *Les races humaines*, Paris, 1944, p. 8.
- <sup>2</sup> Cedric Dover, *Know this of Race*, London, 1939, p. 12.
- <sup>3</sup> Guido Casaliini, s.v. 'Razza,' *Dizionario di medicina*, Turin, 1939, Vol. II, p. 485. On p. 1117 of Ottorino Pianigiani's *Vocabolario etimologico della lingua italiana*, Rome and Milan, 1907, Canello is credited with suggesting *ratio* as the etymological base of *razza*.
- <sup>4</sup> All but Pianigiani cited in Eugen Oberhummer, 'Herkunft und Bedeutung des Wortes Rasse,' *Anz. Akad. Wiss. Wien*, P.-h. Kl., Vol. LXV, No. 18, 1928, p. 205; and 'Die Herkunft des Wortes "Rasse",' *Z. Rassenk.*, Vol. I, Part 1, 1935, p. 92. I am indebted to Mr. I. G. Gunnison for translating Oberhummer's more recent paper and that by Kälin referred to below.
- <sup>5</sup> John Small (ed.), *The Poems of William Dunbar*, Edinburgh and London, 1888-1893, 3 vols., Vol. I, p. clxiii, and Vol. II, p. 118. This is the version preferred by the O.E.D.; another ending 'in secretis placis' is quoted in J. Schipper, 'The Poems of William Dunbar,' Second Part, *Denkschr. Akad. Wiss. Wien*, P.-h. Cl., Vol. XL, Part 4, 1892, p. 40, and in W. Mackay Mackenzie, *The Poems of William Dunbar*, Edinburgh, 1932, p. 121.
- <sup>6</sup> Dover, *Know this of Race*, p. 64.
- <sup>7</sup> E.g., E. von Eickstedt, *Rassenkunde und Rassengeschichte der Menschheit*, 2nd edn., Stuttgart, 1937, Vol. I, Part 1, p. 21; J. Kälin, 'Zur Systematik und Nomenklatur der fossilen Hominiden,' *Bull. Schweiz. Ges. Anthropol.*, Vol. XXI, 1944-45, p. 64 (noticed by me in *Eugen. Rev.*, Vol. XXXVIII, No. 2, 1946, p. 97); a number of memoirs by G. Bonarelli in his *Ultima miscellanea*, Gubbio, 1944-46, Vols. I and II; and K. Modrzewska, 'Professor Frassetto's New Classification of the Races of Fossil Man,' *Przegląd antrop.*, Vol. XVI, No. 4 (1950), p. 320 (in Polish).
- <sup>8</sup> *MAN*, 1949, 155.
- <sup>9</sup> The Hon. Editor fears that some readers may have read too much into his comment (*MAN*, 1951, 32) that 'such an *ex cathedra* statement to the world must clearly be such as to command the assent of all recognized authorities.' Far from being an expression of starry-eyed optimism, this was intended to mean that a document sponsored by U.N.E.S.C.O. and purporting to state only established facts must necessarily be confined to views commanding such assent, even if this results in a 'weak' statement—or none at all.—Ed.
- <sup>10</sup> Cedric Dover, 'Not Genetics but Pettersson,' *Mod. Educ.*, Vol. IV, No. 1, 1950, p. 8.
- <sup>11</sup> Carl L. Hubbs, 'Criteria for Subspecies, Species and Genera, as Determined by Researches on Fishes,' *Ann. N.Y. Acad. Sci.*, Vol. XLIV (1943), Art. 2, p. 111.







(a) (b)



(c) (d)

(e) (f)



(g) (h)

(i) (j)



(k) (l)

(m)



(n)

BUSH NEGRO CALABASHES FROM SURINAM

Photographs by P. J. C. Darke



# SOME NOTES ON THE CARVING OF CALABASHES BY THE BUSH NEGROES OF SURINAM\*

by

PHILIP DARK

## Introduction

97 Bush Negro culture appears to be extremely well integrated. The Bush Negro lives in an exacting environment to which he has become very well adjusted. Historically he has resisted with great success all attempts to incorporate him, as a subservient member, into the Western European form of civilization. This highly integrated quality of Bush Negro culture seems to find expression in the underlying cohesive quality of their art. Outwardly it shows a lack of consistency and apparently an unlimited range of designs, but the cohesive quality that this art engenders is confirmed to a large degree by a detailed examination.

An aspect of Bush Negro culture which shows a certain lack of permanence is that of marital relationships: the women appear to have the whip hand in these matters and divorce is quite an informal matter, a woman being able to break a union on some slight pretext. As a result the husband is in the position of a permanent suitor, and to maintain the affections of his spouse he woos her with gifts of carvings. The women are evidently highly gifted connoisseurs of the art of carving and appreciate a new piece that is excellently executed. The carvings carry a high degree of sexual symbolism, frequently of an extremely personal nature. They also form part of the bride-price. A carving carries the same significance when given by a boy to a girl as when presented by a husband to his spouse. The variety of designs employed by the Bush Negroes, together with a lack of repetition in their art, seems to reflect this aspect of social behaviour. The underlying consistency of the art, on the other hand, seems to reflect a wider form of social integration.

In the field of aesthetics, then, the role of the men is primarily concerned with creation, that of the women with appreciation. The only significant contribution made by the women to art expression is in the decoration of calabashes. (The women evidently do a little embroidery; the only textiles woven are cotton arm and leg bands, and pottery is a very crude utilitarian black ware; these crafts show a very poor development.)

## Classification

Recently I examined a number of Bush Negro calabashes in two collections, those of Professor M. J. Herskovits and of the American Museum of Natural History, New York.<sup>1</sup> I examined Dr. Herskovits's collection of 32 calabashes first and two distinctive features were apparent,

\* With Plate E and three text figures

the first based on form, the second on surface. As to form, calabashes can be divided into those having the forms of a hemisphere and of a spherical intercept. As to surface, some calabashes are carved with designs on the outside, the inside being left plain; others are carved on the inside, the outside being left plain.

On closer examination they were found to fall into four categories, based on form. These were designated: (i) hemisphere, (ii) ovate hemisphere, (iii) 'hemisphere plus,' and (iv) spherical intercept (fig. 1). Calabashes

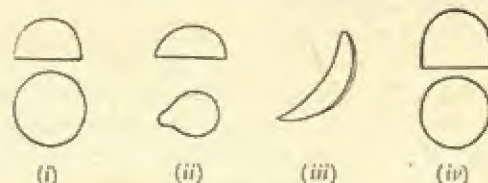


FIG. 1. CALABASH FORMS

(i) Hemisphere, (ii) ovate hemisphere, (iii) hemisphere plus, (iv) spherical intercept

decorated on the outside are carved with curvilinear designs, a type of design which is found on other Bush Negro carved objects, such as combs, food-stirrers and stools. The designs used to decorate the insides of calabashes are more angular in nature; the range of motifs employed is different from those used on the outsides and is atypical of the general range of design elements used in Bush Negro carving. I further noticed that those calabashes with designs on the outside had been cut with the stem join at the top of the hemisphere; either section resulting from this type of cutting may be decorated. Calabashes decorated with designs on the inside only are all cut longitudinally.

I subsequently tested and confirmed the above categories on 79 calabashes in the Museum of Natural History. Of a total of 95 calabashes, 15 had designs on the outside only: four hemispheres (i), six ovate hemispheres (ii), and five hemispheres plus (iii). These, it should be noted, were carved by men. Of the remaining 80, carved by women, 16 were of category (i), 18 of (ii), and 45 were spherical intercepts (iv). Thus it would seem that women do not as a rule carve the type which I call hemisphere plus.

The designs employed on the outsides of calabashes are in the nature of low reliefs, the motifs being related to those used on combs, food-stirrers and other Bush Negro carvings made by men, and consisting primarily of intertwining spirals, zigzags or combinations of the two. The surface around a motif is elaborately incised (see fig. 2a) in



a manner consistent with having been done with a finger nail. This would necessitate a calabash being decorated when soft, and it may be that the decoration of calabashes is done entirely in this manner. The designs on the outsides

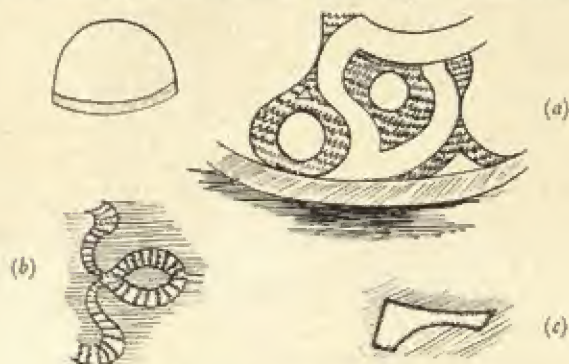


FIG. 2. CALABASH DESIGNS

(a) With zigzag incision of background and clearly marked rim, (b) with all-over zigzag incision of motif, (c) the 'bone motif'

never overlap the rim, which is always clearly marked (see fig. 2a). The design is always conceived as having to fill a definite surface and the relationship of design to design space is always well considered and apt.

The designs on the insides of calabashes are mainly of two kinds: one is more angular than those of the men; the other, though curved or circular in quality, is more restricted in flow than those of the men. Both convey a certain quality of feeling that is alien to the general range of designs employed by the men. Both, also, manifest a strong sense of the segmentation of individual elements; this sense of designing is alien to that of the men. The motifs are incised on the surface rather than carved in low relief. Some of the designs on the insides are symmetrical and are evidently conceived as having to fill the space they are to decorate, while others are asymmetrical. Still others convey the feeling that the artist was able to fit only a section of the motif conceived into the area to be decorated—a feeling somewhat similar to that obtained from certain Maori carvings, particularly the circular motifs on canoe prows where the design is suggested as continuing in space.

#### Description of Typical Specimens

Some examples of calabash designs are shown in Plate E. The first two, (a) and (b), seem fairly typical of designs employed on the insides of calabashes carved by the Saramaccaner tribe of Bush Negroes, and are examples of the symmetrical type of women's designs. The design of (c) is an exception to the type generally found on the insides of calabashes; it appears to come from the design range of the men, though it is not conceived as filling the space to be decorated, but rather overflows the boundaries of the form into space. This may therefore be a case of a woman employing a design from the men's range. Another Saramaccaner piece, (h), is a watertight container, consisting of two calabashes, an ovate hemisphere and a hemisphere *plus*, fitted together. Both are carved on the outside with typical men's designs. Pieces (e), (f) and (g) are examples from the Aucaner tribe; (g) depicts some sort of

bird, an exception among designs found on the insides of calabashes; (e) seems also to be an exception, for the depiction of a human being, as this seems to be, is rare in this art. It would seem that of the designs used to decorate the insides of calabashes those used by the Aucaner may be, on the whole, more flowing or circular than those of the Saramaccaner, which convey a more angular feeling.

A common motif is the 'bone motif,' seen on both the inside of (l) and the outside of (n). The low-relief effect on the second example is obtained by zigzag incision along the inside of the border of the motif (see fig. 2c), similar to the background incision which surrounds the motives on calabashes carved by men. In this case the background is unincised; the inside surface is decorated in the same manner. A similar form of incised design, covering the whole motif, is seen on (m) (see also fig. 2b). The unincised background is smooth and has acquired a dark brown colour. The designs on both outside and inside seem typical of those used by the men; the background surfaces inside are incised in the same manner as exterior surfaces are normally incised on calabashes carved by men. Another such specimen is in the New York collection.

The bone motif also appears on (i) (see fig. 3a). Here, however, only part of it appears in the space to be decorated, and the eye must travel into space to create the complete motif. Fig. 3b and c give further examples of the

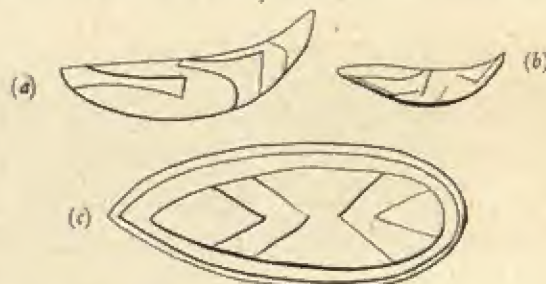


FIG. 3. SPHERICAL INTERCEPT FORMS

(a) With 'bone motif'

designs used on spherical-intercept forms. Fig. 3c illustrates a trait noted as typical in the decoration of calabashes by men, namely the demarcation of the design space by additional lines, parallel with the edge, to emphasize the limits of a form.

#### Discussion

From the data collected, then, and allowing for the few exceptions which I have mentioned, it would seem that the Bush Negroes employ two distinctive styles of decoration on their calabashes: the men's style, which appears on the outsides of calabashes; and the women's, which appears on the insides. Of the men's style the distinctive qualities are: (1) intertwining, curvilinear motifs; (2) zigzag incision of the surface around a motif, conveying the feeling of low-relief carving; (3) clear demarcation of the rim or design space over which the design never goes; (4) designs always conceived as having to fill a definite surface; (5) an apt consideration of the relationship of design to design space; (6) designs falling within



the range of designs used by the men in their carving of other objects. Typical examples of the men's designs are Plate *Ec*, *h* and *m*, and fig. 2*c*. The distinctive qualities of the women's style are: (1) motifs which are either more angular, or, if curved or circular, more restricted in flow than those of the men; (2) general absence of zigzag incision of designs; if it does occur, then it would seem to be on the inside of a motif and only round its edges; cutting into the surface is incision rather than low-relief carving; (3) rims generally not marked; the design may or may not overflow the design space; (4) designs which may or may not be conceived as having to fill a definite surface; (5) relationship of design to design space which may or may not be apt; some designs are symmetrical, others asymmetrical, and still others show no consideration for design space; (6) a strong sense of the segmentation of individual elements of a design; (7) a quality of feeling from the design range that is alien to that from the general range of men's designs. Typical examples of the women's designs are Plate *Ea* and *l*, and fig. 3*a-c*.

The Bush Negroes probably used these carved calabashes for a variety of purposes. The literature on the subject says they were used as bowls, plates, spoons and watertight containers. Clearly, only type (*iv*), the spherical intercept, would serve as a spoon.

The phenomenon of two different art styles coexisting in a single culture is manifested in a number of areas. Dr. Linton has written ('Primitive Art,' *Kenyon Review*, Vol. III (1941), pp. 34-51): 'It is [the] rigid ascription of occupations which is responsible for the not infrequent phenomenon of two or more totally different art styles coexisting in a single primitive society with little or no influence on each other.' He cites as an example the art of the North-West Coast where the style of the art done by the men is curvilinear, in contrast to that of the women, which is angular and geometric. Further, 'A somewhat similar situation was present in Plains Indian art where angular geometric designs and a vigorous naturalistic representation of men and animals flourished side by side. Here again the geometric art was the work of women, the naturalistic art that of men.' A comparable situation is also apparent in the art of the Maori of the Northern Island, where the women made cloaks with geometric designs in contrast to the curvilinear style of the carvings of the men. The emphasis on certain aspects of the forms decorated by women is in distinct contrast to the emphasis put on designs by the men.

The art styles of the men and the women of Bush Negro culture are a further example of this phenomenon. But they also present us with certain contrasts to the other cases mentioned above. For Bush Negro art, considered as a whole, manifests an overall style which an observer would recognize as the Bush Negro style: the men appear to select from the conceptualization component of this style a certain range which is in contrast to the range chosen by the women. If a style is considered as made up of smaller elements called themes, then the men and the women in Bush Negro culture have apparently selected two distinct themes of artistic expression on which to elaborate.

Further, as I have said, the business of creating works of art is primarily that of the men; the appreciation of them is primarily the concern of the women. Thus in the overall consideration of Bush Negro aesthetics there is, generally speaking, a strong tendency to a division on sex lines of aesthetic activities. The carving of calabashes by the women is an exception; nevertheless, the conceptual processes involved would appear to be different and to strengthen this apparent difference in aesthetic activities between men and women. Bush Negro art, then, presents us with a case of men and women working with the same materials to provide similar results as regards forms and use, but decorating these forms with designs from two themes of a common art style that are alien to one another.

We have seen that many, probably the majority, of the women's designs manifest no consideration of the limits of the shapes they decorate. It is tempting to consider that this phenomenon reflects a distinct difference in temperament between the men and women of this culture. The men would appear to be definite in their actions and probably more conservative than the women. The women may perhaps be thought more fickle and more easily susceptible to novelty. Perhaps the decoration of calabashes by the women is their form of doodling.

The possibilities of predicting the social aspects of a culture with any accuracy from artifacts seem extremely limited. An apparent variability expressed in one category of artifacts is not necessarily an indication of a culture poorly integrated in its social aspects. Variability expressed in a number of categories of objects would appear to reflect a more poorly integrated culture, but clearly this is not necessarily so. Highly subjective qualities underlying an apparent lack of elements held in common in a range of objects may, however, provide a clue as to the quality of integration of a culture; but in the whole problem of reconstructing a culture this aspect would seem to offer contributions of but a highly dubious nature. It would seem, however, as I have suggested, that certain correlations between the general nature of a culture and the character of its art might be made for Bush Negro culture; further, that certain more specific correlations might be made, such as the apparent reflection of the variability of Bush Negro art in the tenuous quality of marital relationships.

The suggestion that differences in temperament between Bush Negro men and women are reflected in certain forms of their respective art styles would appear to be paralleled in Maori culture. The differences in designs and sense of design space<sup>2</sup> between Maori men and women has already been mentioned. The arts of tattooing and hair-dressing again seem to reflect a difference in temperament. The tattooed designs of Maori men are elaborate and of a flowing curvilinear quality; those of the women are simple, often only a few simple marks of a geometric nature. Maori men seem to arrange their hair by an extremely elaborate process; they appear to be more flamboyant and fond of display and more susceptible to change than Maori women, who in turn seem more conservative and less extrovert in temperament.



Such examinations as this of differences in the art forms of various cultures, together with more detailed analyses, may provide a profitable source for the study of certain aspects of both æsthetic and social behaviour. Further, they might be helpful in determining the æsthetic components of personality configurations, which seem to have been a rather neglected aspect of these studies.

## Notes

<sup>1</sup> I wish to acknowledge the kindness of Dr. Herskovits and of the American Museum of Natural History for permitting me to study their collections of Bush Negro art material.

<sup>2</sup> It is this difference of conception of design space, a feeling for a specific configuration of elements forming a style, and the emphasis laid on this, that is of prime importance. It is acknowledged that certain techniques limit the type and range of design.

## A YAO GIRLS' INITIATION

by

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**98** The following is an account of a ceremony which I witnessed in a Yao village in the Dedza District of Nyasaland while I was engaged in field work at the headquarters of a Cewa chief some three miles away. It is necessarily superficial, as my main work was with the Cewa and I had few contacts with the village in question. Also the ceremony cannot be regarded as typical of these rites among the Yao, since in this region of mixed tribes there has been much assimilation of neighbouring cultures; in particular the Nyau dances, which played a prominent part in the proceedings, belong to the traditional culture of the Cewa and not the Yao. Since, however, men may not witness these rites, even a superficial description of a non-typical example may be of some value.

The ceremony is known as *ciputu* and the girls initiated as *namwali*. (Where I use African words, they are those of my Nyanja-speaking informants.)

Whereas the Cewa consider that a girl is ripe for initiation when her breasts begin to form, the Yao hold that the right age is earlier, about nine or ten; the communal instruction given then is supplemented by individual instruction at first menstruation, and again (as also with the Cewa) at the first pregnancy. The actual age in particular cases of course varies, since the dances (as the whole ceremony is always called) are not held every year. There must be enough girls of suitable age to make it worth while, and their parents must feel that they can afford the fee due to the *namkhungwi* or mistress of ceremonies, and that they have enough grain for the beer and feast of maize-meal porridge which are consumed at the opening and closing rites.

The right to hold the dances is inherited from an original holder who obtained it by a grant of the territorial chief. According to Mr. Clyde Mitchell, although the right to hold the boys' dances goes with the village headmanship, by matrilineal descent, a man can bequeath the girls' dances to his own son. The concrete symbol of the right is the small shallow basket (*nsengwa*) from which flour is poured to offer to the ancestors at the beginning of the ceremony. The prayers are spoken by the headman and the flour poured on the ground by a sister's daughter of his.

Each *namwali* has a 'counsellor' (*phungu*), who takes a special responsibility for her throughout the initiation period. The counsellors are women already initiated; an elder sister, or the woman who named the girl at her birth (any relative who is at hand at the time can do this), is suitable for this office. At the ceremony I saw, all the counsellors were quite young women.

The preparations consist in the building, by the men of the village, of a rough grass shelter (*masasa*) for the girls to sleep in during the period of instruction, and the brewing of beer. From the time when the grain is put to soak for the beer, those who wish to be counsellors must refrain from sexual intercourse, as must the parents of the initiates.

Ciunda's ceremony began on a Saturday evening. This is now common form for any celebration at which a large concourse is desired. In the case of those which men attend the reason will be obvious; and since most pagan women now observe Sunday as a day of rest from hoeing, the weekend is the best time for them too.

Ciunda pointed out to me the niece with whom he had made the sacrifice to the ancestors, but I did not see this. According to other informants, it is offered before the *lupanda*, a small forked twig about twelve inches long which is stripped of bark and coloured with black stripes, and, at the opening of the boys' ceremony, is set on the ground in front of the *masasa*. After the prayers have been said a little of the flour is smeared on the forehead of each *namwali*.<sup>1</sup>

The complete ceremony involves two public rites, one at the beginning of the seclusion period and one when the girls return to the village. It is the first of these that I am about to describe.

When I arrived about 8 p.m. the 13 *namwali* were seated on mats in groups of three or four round two small fires. I was variously told that they all belonged to Ciunda's village and that some had come from neighbouring villages. One had breasts just visible, another breasts rather more fully formed, the rest were younger. They wore the costume prescribed for the occasion—strings of beads round the waist (in one case a woven bead belt), a piece of cloth between the legs with the two ends passed



through the lower rows of beads and hanging down in front and behind, and another tied round the waist above the beads and knotted in front. A week or so before I had seen two girls dressed like this in the village; I was told that some like to try it on in advance.

The girls were seated in silence with their legs stretched straight out in front of them and their eyes modestly downcast—the ritual posture also adopted by people sitting by their gardens when prayers are being offered at the rain shrines, and, Mr. Mitchell informs me, by a village headman at his installation. Each little group had a cloth or blanket covering their heads and shoulders, not for concealment but for warmth; it was quite a cold night. The *mapungu* rearranged these from time to time. Though the girls moved their heads sometimes, during a period of about an hour I only saw one curl up her legs.

Women were walking round the fires singing, clapping their hands and uttering the 'ululu' shriek of joy. Two or three times somebody in the distance fired a shotgun; each time this was the occasion for redoubled shrieks. A little further off a group of younger women were dancing with the enthusiastic encouragement of a young man, whom I recognized as one recently returned from the gold mines. When I expressed surprise at his presence I was told that this was a 'mere dance' and not part of the ceremony; I did notice, however, next day, that he was the only man, apart from Ciunda himself, to be seen anywhere near the scene of proceedings.

Presently the women sat down near the fire. A voice was heard in the distance chanting words to which they replied with a short refrain. Later the shrill singing of several people was heard, coming now from one direction, now from another.

Then there was a pause while we waited for the moon to rise. The little girls were now allowed to lie down and were covered with the blankets. When the moon was held to be high enough, the voice of the *namkhungwi* was heard calling them to go to the *masasa*. Each *namwali* was lifted up and carried on the back of her counsellor, and the procession set off in single file, followed by the girls' mothers, each bringing a basket of flour for food for her daughter during the period of seclusion. When it arrived at the *masasa* it was met by three or four old women, all, I was told later, *namkhungwi*; one of these wore a crownless hat of straw and feathers such as is worn by the *namkhungwi* at the boys' ceremony. When the *namkhungwi* had danced for some time, the counsellors let their charges stand on the ground, and each in turn was lifted up, lying horizontally, and raised on the hands of three or four women high above the heads of the crowd, and then carried to the *masasa*. I left at this point; the dancing is kept up till dawn.

Though some informants insist that the girls are not allowed to sleep at all at night, nothing is done to prevent them from sleeping if they can sleep through the noise; but they do get much less than normal sleep. The instruction does not begin till after the Sunday's dancing; early on the Sunday morning they are taken out by their *mapungu* to 'play' in the long grass.

I returned about mid-day on Sunday to find the

*namkhungwi* dancing outside the door of a house, surrounded by a circle of women who provided them with an orchestra by clapping and singing. Although it is not easy to see any particular rhythm in these dances, the accompaniment of clapping is held to be so essential that on another occasion a troupe of boys whom Ciunda had sent to perform at a boys' initiation were asked to stop their evolutions because they were distracting the women from their duty of 'helping the dancers.' A number of complicated rhythmical effects can be produced by clapping the hands first flat and then hollowed, and the rhythm of the phrases sung cuts right across these. Whereas men dancers are encouraged by having pennies thrown to them, on this occasion one of the dancers distributed pennies among the clappers. I was surprised to find at the end of the day that I had earned 5d. Some women also received small quantities of maize. The *namkhungwi* danced outside the hut of each woman who had a daughter being initiated; if there were any girls from other villages, however, their mothers did not receive this compliment.

At 2 p.m. the whole crowd repaired to the *masasa* to await the return of the *namwali* and their counsellors. Apart from the fact that men must not approach it, there appears to be nothing sacred about the *masasa*. When we reached it various people were taking advantage of the shade it provided to sit inside and eat.

There was now about half an hour's dancing. The leading *namkhungwi* appeared carrying on her head the sacred basket tied up in a cloth. I was told that it now contained horses' tails, which are much used in these parts for magical purposes. Various women in turn seized this, put it on their own heads and pretended to run away into the long grass with it. Then another dance was performed in which the women appeared to be simulating decrepitude—hobbling with hands held against their backs, dancing with stiff legs and so on. For the final minutes of the dancing the circle of spectators knelt on the ground and beat it with their hands at every third beat.

Mats were now spread on the ground for the *namwali* to sit on, and they arrived, carried as before on the backs of their counsellors, and with their faces pressed against them. During the next part of the proceedings, which lasted about half an hour, all the *namwali* had to keep their eyes closed so as not to see the *namkhungwi*. When they were set down, each counsellor held the head of her *namwali* pressed against her chest. Next they were lifted one after another above the heads of the crowd as on the previous night. After this they sat down on the mats, each little girl supported by two women. These two sat face to face with legs extended, one laying her legs across those of the other. The *namwali* sat between the legs of one of the women, her own legs extended just inside the latter's and with her head bent forward against the other's chest. Most had their heads clasped in the hands of the woman facing them. The rest of the women walked round and round them singing; the excitement was redoubled when one of the *namkhungwi* began to go round in the opposite direction. When the dancing ceased the girls were told to hold their hands stretched out behind them, and one *namkhungwi* rubbed



their palms with a corner of the cloth she was wearing, which evidently contained some medicine, while another put her finger on each girl's head. Each counsellor then rubbed first the hands, then the head and back of her *namwali*, apparently in order to spread over her the medicine which the *namkhungwi* had rubbed on her hands. Meantime the spectators clapped their hands and shrieked and there were shouts of 'Namwali! Hooray!' The purpose of the magic is to protect the girls from the danger of sorcery during the initiation period; it is not held to reinforce the qualities which their instruction aims at producing.

At 3.10 the women who had been supporting the girls got up, and the girls were arranged in two rows, sitting with legs outstretched as on the night before. The spectators adjourned to partake of a meal of maize porridge, and Ciunda appeared. He had told me earlier that he was the only man allowed to approach the *masasa*, and could even go inside; it should be mentioned that in addition to being the headman he is himself a *namkhungwi* of some renown for the boys' ceremonies, and claims to have taught his sister some particularly effective magic which he bought from the *namkhungwi* who initiated him.

I was now told that this part of the proceedings was over, and when I looked again at 4 o'clock I found the little girls lying down where they had been sitting, completely covered in blankets.

A few minutes later shrieks were heard again, and on a ridge in the distance could be seen two huge grey figures slowly approaching. These were the 'monsters', woven of grass and the sheaths of maize cobs, and each carried by two men, which form the central feature of the Cewa Nyau dances. This is a feature of the ceremony which would not be found where Yao are not in close contact with Cewa. As women are not supposed to see these figures by daylight (in order to maintain the fiction that they really are wild animals) they would remain hidden at a ford of the river till dark and then come to dance at the village to 'frighten the girls and keep them awake.'

The dances would be held, I was told, on three successive nights while in the daytime the 'animals' would hide by the river, appearing from time to time in the distance as on this first occasion.

On the Monday night I went with a Yao woman and her daughter from my own village to see the dances. It was a moonless night which later became overcast. About a hundred yards from the *masasa* my guide insisted that our lantern must be extinguished, and we stumbled the rest of the way in complete darkness. We arrived to find the dance going on just outside the *masasa*. Drumming, singing, clapping and the noise of rattles and the clashing of two metal objects (probably parts of a dismantled bicycle bell) could be heard. This time the women did not form a circle, but kept to one side of the dance floor, sometimes advancing in single file towards the dancers.

After a time there was a stir of excitement and I was told that 'the animals' were coming. All that could be seen in the darkness was their towering height and the two tall points ('horns') with which each was crowned. They did

not in the least resemble any real animal, but equally they resembled nothing in ordinary experience. They spun round and round and from time to time advanced towards the crowd with a rush like a charging animal before which the women recoiled in real fear. (I was told that I must not sit down because I must be ready to run.) The figures were produced twice during the two hours while I was there.

In an interval in the dancing a woman whom I did not recognize seized me by the hand and led me to the other side of the dancing floor, where three others were standing. She told me that only they had the right to be there, and that they had bought it with money. Next day I was told that they were the *namkhungwi*.

In pauses between the dances the men—of whom there were perhaps six or eight—uttered strange sounds vaguely resembling animal cries, and chanted phrases in distorted voices to which the women sang replies. At one point the woman beside me muttered, 'No, no.' When I asked the reason she said, 'They've got the song wrong.' At another point they chanted several times, 'Namwali! Ee!'

When somebody pulled aside the reed door of the *masasa* and the firelight streamed out, the men quickly moved out of the beam into the darkness. All are supposed to wear masks and have their bodies smeared with earth of different colours; while standing with the *namkhungwi*, however, I saw two men in shirts and trousers.

In another interval the whole crowd gathered close to the *masasa* and I was told that 'the animals' were punishing those girls who had been disrespectful to their elders. It is said that they are slapped and kicked. On the way home my guide asked her daughter who had been punished; it appeared that only one girl had been judged to deserve this.

The girls spend a week or sometimes two sleeping in the *masasa* and concealed in the long grass in the daytime. After the first day's celebrations their beads and loincloth are removed by the *namkhungwi* and each is wrapped in a blanket which covers her from head to knee to prevent her being seen by a man. If one approaches she must draw the cloth over her face and run away.

During this time they are instructed by the *namkhungwi*. On the content of this instruction I have unfortunately only the sketchiest information. It is said to concern 'work, caring for one's mother, caring for one's husband, bringing him water to wash, politeness.' Whether express instruction on the sexual process, and on the occasions when intercourse is forbidden and believed to be dangerous, is given in the girls' as it is in the boys' initiation, whether it is deferred till the ceremony on first menstruation, or whether it is given on both occasions, I do not know. I was assured that there is no operation, though Mr. Mitchell's informants described one to him. I had to leave the district before the final ceremony, the essence of which is the triumphal return of the girls and their welcome back to the village. They are taken to the river to wash all over, are anointed with oil and have their heads shaved and are dressed in barkcloth, which today has to be bought from one of the rare experts who still make it. I was told that these proceedings are not accompanied by magic such as I had seen at the corresponding ceremony for boys.



These rites are apt to be condemned out of hand by the representatives of missions, though I have heard the opinion expressed that they have their valuable elements but must be suppressed on account of the objectionable ones.<sup>2</sup> Africans of all the Nyasaland tribes which practise them unanimously, and without prompting, describe them as 'our schools.' The unprejudiced spectator cannot fail to be struck by their educational features. The importance of the occasion is impressed on the girls in every possible way — by the rejoicing of the older women, by the solemn demeanour which is required of themselves, by the elements of physical discomfort, by the rites which they must go through with closed eyes, not knowing exactly what is going to happen, by the mysterious and alarming Nyau performances, by the public chastisement of those who have behaved badly, and by the formal exaggerated avoidance of men, stressing that they are now too old to play freely with boys. No Education Department has at its disposal such effective devices for enhancing its prestige. One would have had actually to follow the instruction to decide whether it contains obscene elements; many people

today do not consider that the mere mention of sex is obscene, nor that it should not be mentioned to immature persons. The question whether the dances have an obscene significance, again, could not be answered without careful research; the assumption that they have is certainly made too readily.

#### Notes

<sup>1</sup> Mr. Mitchell writes: 'There are three sacrifices done by the owner of the basket, at Kawinga's at any rate. (1) He puts a pile at the shrine-tree and asks the ancestors if the time is propitious for an initiation. If the cone of flour collapses this is an indication that the time is not right. This is done the night before the initiates go to the *masakasa*. (2) He goes to the *masakasa* in the afternoon after the spirits have agreed to the ceremony and invites them at the *lipanda*. (3) He smears the forehead of each initiate and the forearm of each mother the evening of going to the *masakasa* [the Yao word].'

<sup>2</sup> This view doubtless has in mind the defloration of the girls which in the traditional rite was performed at the end of the period in the bush. I have no information about this. Cewa informants tell me that it is being abandoned among them, and in a case on record in one native court a bridegroom successfully claimed damages for adultery against the man appointed by her relatives to deflower his bride.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**The Sacred Dance in Indochina and Indonesia.** By Mlle Jeanne Cuisinier. Summary of a communication to the Institute, 24 October, 1950

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Dances may be considered sacred when they are part of a ritual, when they have a magical object, or when their religious origin can still be traced. Most dances can be placed in one or other of these categories. By dancing a communion is established between humanity and the supernatural world, between the performers, and between those and the spectators. From a sociological point of view it is possible to distinguish between popular, royal, religious and magical dances, which are not however rigid categories.

(a) Popular dances can be divided into those regarded as entertainment and those with a ritual purpose; the ritual dances into those accompanying domestic and those accompanying public ceremonies; and the latter into those associated with peaceful activities, such as agriculture, and those associated with war. There is also a distinction between dances performed by the ordinary people of a village, and those performed by professional or specially trained dancers. (b) Royal dances may be divided into those performed by princes, either for pleasure or with a ritual purpose, and displays by royal troupes of varying composition. A dance performed by the ruler with a ritual purpose also comes into the religious category. Dances performed by royal troupes are often imitated in the popular dances. (c) Religious dances may be liturgical, performed by priests or priestesses or by rulers acting as such for the time. Other dances accompanying religious ceremonies often cause in the performers a state of ecstasy, which may be induced by drinking or inhaling intoxicating substances. The distinguishing feature of the religious dances is that they are always a form of homage to the divinities or to the dead; they therefore include funeral dances. (d) In discussing the distinction between religious and magical dances, the lecturer quoted Marcel Mauss on the individual nature of magic: 'we call magic any rite which is not part of a cult'; but magic may be utilized for social ends.

The lecturer then described and illustrated a number of dances, mainly from Siam, Cambodia and Kelantan, and discussed their significance in relation to this classification.

**The Date of the European Megalithic Tombs.** By Dr. Glyn E. Daniel. Summary of a communication to the Institute, 6 March, 1951

100

Varying dates are given for megalithic monuments from the sixth millennium B.C. to the present day. This is largely because it is assumed that all megalithic monuments are connected with each other. It seems more probable however that the use of large stones for walling and roofing tombs was a device used in different places in the world in different times, with no necessary connexions. Thus the only thing that can be done in dating megalithic tombs is to date accurately regional groups. In this way Wheeler has dated the Deccan megalithic tombs to the last two centuries before Christ, and Reygasse and Gsell the Algerian megalithic tombs to between the third century B.C. and the third century A.D.

Even if we restrict the problem of dating megalithic tombs to that of dating the Western European megalithic tombs, we find dates from the sixth millennium to the beginning of the Christian era. Yet imports from the eastern Mediterranean (such as segmented faience beads, anchor ornaments, decorated bone tubes, bossed bone plaques, etc.) show that megalithic tombs were in use at least by 1500 B.C. Stratigraphical evidence in Spain and the south of France suggests that the tombs were used considerably before this. The arguments of Leisner and Pericot and others for a date before 2,000 B.C. were examined but approval given for Almagro's date of 2,000 B.C. for the first western European megaliths. It was suggested that the Passage Graves of France, Ireland and Scandinavia might date from 1900-1800 B.C. and that the *floruit* of megalithic tomb-construction in France and Britain was from this date through to 1000 B.C. (or even later in some cases).



## SHORTER NOTES

U.N.E.S.C.O. and Race. Cf. MAN, 1950, 220 and 1951, 28-32 and 93-96

**IOI** Early in June there will assemble in Paris a gathering of distinguished physical anthropologists, geneticists and other human biologists which may lead to results of importance far beyond the confines of anthropological science. If their deliberations result in general agreement on any considerable body of 'established fact' about the physical aspects of race, then a sure foundation will have been laid for the praiseworthy and necessary campaign which U.N.E.S.C.O. has undertaken for the abatement of racialistic ideas by the propagation of truth in the form of the findings of science. If U.N.E.S.C.O. has any function to perform at all—and the governments of the world decided after the war that it had—then surely this must be one of its pre-eminent tasks. Admittedly the Organization is ill fitted to conduct research itself, or to tell scientists how to go about their business; its task lies rather, first, in the field of collation of results that would otherwise remain uncollated (as by assisting the compilation of bibliographies or of reviews of progress in given branches of study—in which latter category the forthcoming meeting may perhaps be placed), and, secondly, in the essential work known in French as *vulgarisation*, the dissemination of knowledge in a form assimilable by the masses of men. U.N.E.S.C.O. has then great opportunities for the propagation of truth, and an equally great responsibility for avoiding the propagation of error.

Readers of MAN will not be unaware of the antecedents of the new meeting. It is a direct result of the severe criticism aroused, notably through the columns of MAN, by the Statement by Experts on Race Problems issued by U.N.E.S.C.O. in July of last year; this had been drawn up at a meeting organized by the late Dr. Ramos of the Social Sciences Division of U.N.E.S.C.O., with Professor M. F. Ashley Montagu as *rapporteur*. There is no need to recapitulate here the shortcomings of that document; we may recall, however, that most of them were traceable to the manner in which the whole vast field of racial studies, physical as well as cultural, was thrown open to discussion by a small group of philosophers, historians, sociologists and others, only two of whom had any pretensions to competence in physical anthropology. In a group so constituted, each member is necessarily a layman on all but his own specialism; and such a group is then hardly in better case than the twelve blind men whom Till Eulenspiegel beguiled into believing that he had given one of them twelve shillings, so that they went and feasted at an inn until they discovered that the money was purely hypothetical. In the Ashley Montagu Statement, among much that is sound and valuable, are to be found authoritatively worded pronouncements on problems which may not come within the purview of objective science for centuries, if ever.

Yet, in the perspective of history, the document might well be found—if all now goes well—to have acted as a kind of necessary catalyst, to precipitate general agreement on some at least of the essential facts about the biology of race. It may even be that the wisest course was indeed to begin with a somewhat controversial 'pilot' statement, laced with enough heterodoxy to provoke discussion and thought and to ensure a vigorous reaction in favour of the inductive method; in that case, we need only regret that it was launched as a definitive document, as 'the most authoritative statement . . . ever made.'

There can be no possible justification for regarding the criticisms which have been made as an attack upon the U.N.E.S.C.O. campaign against racialism. On the contrary, they reflected the disappointment which was felt when the document proved to be

not the effective weapon which we had looked for, but a broken reed. It was feared indeed that its more vulnerable points might be seized upon by apologists of racialistic policies to ridicule and discredit the whole campaign, if they were not first corrected by scientists whose motives were above suspicion. Although the constitution and terms of reference of the original panel undoubtedly involved serious errors of judgment—notably in the failure to consult even the most obvious expert opinion—U.N.E.S.C.O. deserves great credit first for undertaking the campaign at all, and secondly for the promptness with which, after the publication of the criticisms, the decision was taken to convene a new meeting on lines far more likely to lead to good results.

The new panel has been conceived on a generous scale and great pains have been taken to secure the services of those whose views are likely to carry the most weight in their own countries. Four representatives, covering racial studies from blood groups to anthropometry, have been chosen from the United Kingdom in consultation with the Royal Anthropological Institute, and it may be safely assumed that a document which all four are able to sign will enjoy the approval of British anthropologists in general; the same will no doubt be true of the other countries represented.

Not only will the panel be very much larger than that which met in 1949, but its terms of reference are restricted to the physical aspects of race. We may hope, then, that the statement which it draws up will be firmly enough based to command virtually universal assent; what must remain in doubt until the panel has concluded its work is the extent to which the new statement can be comprehensive. Clearly unanimity will be indispensable and majority decisions valueless. What is aimed at is the 'highest common factor' of the findings of anthropologists and biologists, and this means that any point which arouses disagreement must needs be excluded. The theory of polygenesis, for example, cannot be specifically and entirely rejected (as it was in the Ashley Montagu Statement), since certain reputable anthropologists uphold it. (It is perhaps another matter when we consider the conflict between 'Michurinism' and 'Mendelism-Morganism'; and the panel can hardly be expected to take account of the opinions of those in whose countries science is not free.) In so brief a meeting, there will of course be little scope for any modification in the course of discussion of the views of individual members; it is unlikely that anyone who had long upheld the view that race is a myth would suddenly be converted. Therefore we may expect that the meeting will concentrate on questions of fact rather than of terminology. Some people think that the word 'race' should be proscribed altogether, others that it should be eschewed by anthropologists, others that the general public should be either discouraged from using it or taught to use it in the strict anthropological sense, and others again, more realistically, that there is no harm in people talking about 'the British race' so long as they do not regard it as an exact anthropological concept, capable of being used to separate individual sheep and goats; but it would be a waste of time to argue any of these matters at the forthcoming meeting.

In conclusion it may be suggested that whatever statement emerges from the meeting should be treated as provisional until institutions and individuals not represented at the meeting have had an opportunity of examining it. It will carry overwhelming weight when it has been endorsed by all leading national bodies and, above all, by the International Congress of Anthropological and Ethnological Sciences (which is to meet next year). The columns of MAN will certainly give it due space at the earliest opportunity; and in the meantime, the members of the panel will carry with them to Paris the best wishes of the anthropological world.



**East African Institute of Social Research : First Conference.***Communicated by Dr. Audrey I. Richards, Director*

**IO2** The first conference of fellows and associates of the East African Institute of Social Research took place at Makerere College, Kampala, the headquarters of the Institute, from 17 December to 23 December, 1950. It was attended by Mr. Lloyd Fallers (Busoga), Mrs. Chave Fallers, Dr. Jeanne Fisher (Kikuyu), Mr. Philip Gulliver (Turkana and Karamojong) and Mrs. Gulliver, Professor E. Hoyt (Fulbright fellowship), Dr. Jaques Maquet (I.R.S.A.C., Bunyaruanda), Mr. John Middleton (Lugbara), Mr. Mulira (Research in Luganda), Mr. Philip Powesland (Lecturer in Economics, Makerere College), Mr. Cyril Sofer and Mrs. Rhona Sofer (Jinja Survey), Mr. Aidan Southall (Alur), Mr. Brian Taylor (Butoro), Mr. J. W. Tyler (Zinza), Mr. Edward Winter and Mrs. Winter (Bwamba-Konjo), Mr. Wilfred Whiteley (Government Anthropologist, Tanganyika) (Makua), Dr. A. N. Tucker, and Mrs. E. M. Chilver (Secretary of the Colonial Social Science Research Council), together with the Director, Dr. Audrey Richards (Buganda), and the Secretary (Miss J. M. Fortt).

Papers were read on 'Field Methods and Field Situations' in which individual fieldworkers described briefly the characteristics of their areas, the objects of their research and the methods they felt to be appropriate for such a study. Professor Hoyt and Mr. Philip Powesland contributed papers from an economic point of view. A preliminary discussion followed on a scheme for the comparative study of African political systems in present-day East Africa.

It is proposed to hold such conferences twice yearly.

**International Congress of European and Western Ethnology, Stockholm, 26 August-2 September, 1951**

**IO3** The first circular has been issued (in March) of the Congress which students of European and western ethnology were invited (at the 1938 meeting of the International Congress of Anthropological and Ethnological Sciences) to attend in 1940, and which was postponed owing to the war. The Chairman is Professor Sigurd Erixon and the Secretary Dr. Albert Eskeröd (to whom correspondence should be addressed at the Nordiska Museet, Stockholm). Intending members are asked to apply by 1 May, and papers are to be sent in by 1 June; papers will not be read, but distributed in advance, so that the time may be devoted to discussion—an excellent practice, though one that calls for a full year's notice at least, and not merely five or six months; indeed this ought to be an absolute requirement of all Congresses, except in emergency. Eight subjects of discussion have been adopted in agreement with the International Commission of Folk Arts and Folklore; this practice, much favoured by

U.N.E.S.C.O., often has its disadvantages, and even dangers, but in this case no one is likely to complain that these broad themes are too Procrustean.

It is a matter for great satisfaction that the ethnology of Europe has advanced to the point where it can sustain a Congress of its own, and British ethnologists, who are campaigning through the Royal Anthropological Institute for the filling of the most conspicuous gap among the national museums of England (see *MAN*, 1949, 49), will especially wish the meeting well. But it would be greatly regrettable if European and other ethnology were now to develop along progressively diverging lines, and it is to be hoped that there will be no move to exclude European ethnology from the International Congress of Anthropological and Ethnological Sciences, and that students of it will not feel that their presence in the broader congress is undesired or indeed unnecessary.

**Parenthood in Primitive Society.** By Professor M. Fortes, Cambridge University. Summary of the *in Thurn Memorial Lecture delivered before the Scottish Anthropological and Folklore Society, at Edinburgh, 30 March, 1951*

**IO4** In his lecture following the annual dinner of the Society, at which Mr. Lewis Spence, Vice-President, presided, Professor Fortes said that an important difference between our family system and those of primitive societies was that for us the keystone of the family was marriage—conjugal relationship. For them, marriage was significant chiefly as a means to the end of parenthood. We saw clearly in primitive societies a fact obscured in our system—the fact that there was always a potential or actual conflict between the conjugal bond and the bonds created by parenthood, those of descent and of brother and sister.

Anthropologically speaking, parenthood merely began with physical reproduction. The social and psychological functions of passing on the social heritage and moulding the character of the next generation were specially stressed in primitive custom. To achieve these ends, parents had to combine what might be called discipline, backed with power, with love, backed with moral sanctions.

One result, clearly seen in primitive society, was that the relations of parents and children tended to be ambivalent. There was commonly a feeling of hostility underlying the affection and gratitude children felt towards their parents. Primitive custom often made provision for socially approved outlets for this. We might perhaps be able to learn something from this in regard to some of the problems of adolescence in particular in our society. But, to apply such lessons, what was most urgently needed was more knowledge of family organization in our society, as it was affected by class, occupational and regional differences.

## REVIEWS

## PHYSICAL ANTHROPOLOGY

**Anthropological Papers of Franz Weidenreich, 1939-1948.***Compiled by S. L. Washburn and D. Wolffson. New York (Viking Fund), 1949. Pp. 267*

**IO5** This memorial volume is a tribute to the late Dr. Weidenreich who died in 1948. It is fitting that such a tribute should be paid, for Weidenreich, during the last twenty years of his life, worked indefatigably to develop the subject of physical anthropology. It will not be doubted that his best work in this field is represented by the detailed studies of the '*Sinanthropus*' material which he completed while he was in Peking. The value of this careful and systematic work has been enhanced beyond count by the tragic loss of all the original fossil specimens during the war in the Far East. When war broke out, Weidenreich himself found refuge in New York, where he was given the hospitality of the American

Museum of Natural History. It was during this time that he turned his mind to general problems of human evolution and wrote the papers and essays incorporated in this memorial volume. They do not represent his best work, for compared with his earlier systematic studies they tend to be rather too speculative and polemical. But they give evidence of an unusually active mind, restless in the pursuit of the intriguing problems which still continue to vex the student of human evolution, perhaps a little intolerant of those whose views do not coincide with his own, but intensely eager and full of stimulating ideas. Dr. Weidenreich's death is a severe loss to the science of physical anthropology, but he has left behind him a record of work which will for many years be consulted by workers in this field.

W. E. LE GROS CLARK



**On the Origin of Species by Means of Natural Selection.**

**106** By Charles Darwin. London (Watts), 1950. Pp. xx, 426. Price 15s.

The reprinting of this first edition of *The Origin of Species* should not only stimulate many first readings of a supremely important work, but also, by comparison with the more commonly reprinted later editions, point up some of the ways in which modern evolution theory has solved, or has at least begun to solve, many of the difficult problems to which Darwin addressed himself. For, as Professor Darlington suggests in his excellent Foreword, in an attempt to fortify his argument against contemporary criticisms—and it must be remembered that the very fact of evolution, and not only its mechanisms, was seriously challenged in his day—Darwin came to rely more and more on Lamarckian theory. In this edition there is only slight consideration of the inheritance of acquired characteristics; the emphasis throughout is on natural selection.

Those who, like myself, make this the occasion of their first detailed acquaintance with *The Origin of Species* cannot help being impressed with it as an achievement of scientific reasoning. Not only did Darwin draw on his vast knowledge of variation under nature, of artificial selection, of geology and paleontology; he recognized far more than is generally assumed (especially by his more mechanistic followers) the difficulties involved in establishing his hypothesis. These may be summarized under the following headings: (1) the question of the material nature of inherited variation and its behaviour in transmission; (2) the imperfections of the paleontological record, especially with respect to intermediate forms; (3) the dilemma of accounting for a very great amount of morphological change in a relatively short time, given the small individual variations on which natural selection acts; and (4) the existence of organs of no clear selective value, and the existence of organs of extreme perfection such as the human eye. To some extent Darwin attempted solutions by means of vague principles such as correlation of growth (would this latter now be interpreted as 'simple genetic control of relative growth rates'? ). In our own time, genetics, paleontology, embryology and experimental studies of natural selection have gone a long way toward reducing the enormity of these problems. Very recently the study of cytoplasmic inheritance and of biochemical genetics has begun to set the heredity-environment issue in a more hopeful light.

Like all truly great scientists, Darwin raised as many questions as he answered. In spite of the critical nature of the evidence he lacked, a considerable number of his speculations have been confirmed, and it is necessary to emphasize that his basic argument remains the foundation of present-day evolution theory. By seeing his work in its historical setting we can appreciate its value, both in presenting so well the existing evidence for evolution as a process, and in orienting so fruitfully the course of modern biology.

DAVIDA WOLFFSON

**Lumbar Breakdown and Erect Posture in Man, with Emphasis on Spondylolisthesis and Herniated Intervertebral Discs.** By Frederick P. Thieme. Ann Arbor (Univ. of Michigan Press), 1950. Pp. 44. Price 50 cents

**107** In this admirable study, Dr. Thieme has investigated the relationship between erect posture, with its concomitant lumbar curve, and the occurrence of two forms of lumbar breakdown (separation of neural-arches, and herniated intervertebral discs). These conditions appear to be unique to man, and it has long been assumed that they are in some way due to inadequacies in the mechanism of upright posture and bipedal locomotion. In elucidating this problem, Dr. Thieme has employed both clinical and anthropometric evidence, together with an analysis of the compression forces bearing on the lower lumbar area. Finally, he has brought the matter into

evolutionary perspective by a brief discussion of the development of erect posture in human history.

A survey of skeletal material indicates that the frequency of neural-arch separation (spondylolysis) in modern man is of the order of 6 per cent.; very high figures were obtained for Eskimos (27.4 per cent.) and Lapps (12.8 per cent.), the significance of which has yet to be worked out. The location of the lesion is most frequently (in 79.2 per cent. of 874 cases) at the fifth lumbar vertebra. Onset of the condition occurs in infancy or early childhood, when the lumbar curve is developing; the combination of mechanical stresses (in early walking) and incompletely ossified vertebral bone is undoubtedly of critical importance.

In the case of herniated discs, population frequencies are difficult to determine because (1) skeletal material is of course of no use, and (2) some herniations do not cause pain and would therefore not come to the attention of clinicians. Of cases reported in the literature, by far the majority (89 per cent. of 825) occur at the fourth and fifth lumbar vertebrae. Dr. Thieme comments: 'The mechanics of the lower lumbar area require the intervertebral disc to perform a mechanical role . . . which is lacking higher in the spine. It is the only factor which can account for the strikingly greater incidence of this lesion in this area.'

After analysing the stress situation attendant on lumbar curvature, Dr. Thieme goes on to show that deviant anatomical variation cannot be the typical cause of lower lumbar lesions. He compares a series of 55 normal medical students and a series of 30 cases of spondylolisthesis in terms of several measurements made on X-ray plates, and finds no relevant significant differences, thereby confirming the hypothesis that it is the normal lumbar curvature itself which is the chief cause. That the lower lumbar area is not structurally weaker than other regions of the spine is demonstrated by the percentage distribution of spinal fractures, which show no concentration at the fourth or fifth lumbar.

In discussing the evolution of upright posture, Dr. Thieme points out that apart from the lumbar curve itself, which must have evolved very early in the history of bipedal locomotion, the vertebral column has changed very little under the impact of a radically new type of body mechanics. It is suggested that a slow rate of evolution for that entity, or absence of any appreciable selection pressure, or possibly a combination of these two factors, is responsible.

Considerable as the intrinsic interest of this study is, its general methodological interest for physical anthropologists is even greater. Many problems of human evolution and variation—those connected with the shoulder girdle and the foot, for example—can only be understood adequately in terms of functional analysis. Dr. Thieme's use of a wide range of data, from clinical medicine, histology and mechanics as well as from the more traditional anthropological sources, shows how effectively an integrated biological approach can serve us.

DAVIDA WOLFFSON

**External Morphology of the Primate Brain.** By C. J. Connolly. Springfield, Ill. (Thomas) (U.K. agents: Blackwell), 1950. Pp. xlii, 378. Price £3 12s.

**108** This volume comprises a systematic account of the cerebral hemispheres of the Primates, with particular reference to their sulcal pattern. More than half the book is concerned with the human brain, and the author puts on record many original observations of his own on the variability of the cerebral sulci. The chapters on encephalometry and endocranial casts are of particular interest to anthropologists. In general this book is a welcome record of facts, with a careful and judicious assessment of their probable significance. As such, it will provide a work of reference of considerable value.

W. E. LE GROS CLARK

**Uaxactun, Guatemala: Excavation of 1931-1937.** By A. Ledyard Smith. Carnegie Institution of Washington Publ. No. 588. Washington, D.C., 1950. Pp. 108, 8 tables, frontispiece, 143 illus. Price \$9 paper covers, \$9.75 cloth

**109** This is a final report on the excavations with an interesting introduction by Dr. A. V. Kidder, and it deals with the excavations,

architecture, burials and caches. The pottery, artifacts and the historical, ethnic and cultural background are dealt with in other monographs. The present work covers a period of over 600 years of stone vaulted architectures from their early beginnings to the end of the Classic Period and also covers two prior periods called Pre-Vault I and Pre-Vault II, the two latter being mainly distinguished

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from each other by pottery. The aim has been to give a clear picture of what was found and then to bring out the gradual development and change in various elements. This could not have been done without the aid of ceramics, which the author's brother, Mr. R. E. Smith, in a separate report, divides into four phases called Manoni, Chicanel, Tzakol and Tepecu, corresponding respectively to Pre-Masonry, Pre-Vault (both prior to Maya dated monuments), Early Classical (Vault I, A.D. 278-593) and Late Classical (Vault II, A.D. 593-889), all dated by the Goodman-Thompson correlation of Maya inscriptions. What happened after the date of the last monument must, as the author says, be left to supposition, but he believes that it could not have been many years before all building ceased at Uaxactun. All the evidence points to the abandonment of the city by the ruling class and artisans, and a lingering occupation by hangers-on or wandering tribes without the knowledge or will to hold to former standards, but there is nothing to show whether these were remnants of the former inhabitants, or not.

The portion of the city excavated is thoroughly described, and illustrated by plates. The author deals in a most interesting way with the architectural history of the excavated buildings—a matter requiring close study, on account of the Maya practice of constant change and rebuilding. Many burials were found in the buildings and it is suggested that the edifices were often altered and reconstructed after the burial in them of persons of importance (as evidenced by the wealth of their burial furniture). The suggestion is very interesting and seems supported by the facts. A notable feature of the book is the series of restoration drawings by Miss Tatiana Proskouriakoff. These together with the descriptions in the text make up a fascinating story of the complicated, ever-changing development of the buildings.

The epigraphic material has already been dealt with separately as far as the stelae are concerned, but a quite unique discovery was made of a wall painting showing seventy-two consecutive day signs with their numbers. The number 72 is one-fifth of the *tin* (360-day period). These are the earliest painted day signs known, being probably at least 500 years older than the Dresden Codex, and they are very fully discussed by Mr. J. Eric S. Thompson, who shows how much they add to our knowledge of Maya writing at that early period.

RICHARD C. E. LONG

**Culture and Ethos of Kaska Society.** By John J. Honigmann. Yale Univ. Publ. in Anthropol., No. 40. New Haven (Yale Univ. Press) and London (Geoffrey Cumberlege), 1949. Pp. 368, with 12 plates and 7 figs. Price \$4.

Dr. Honigmann has done much to fill an ethnographic gap in this study of the Kaska, an Athabaskan people of north-western Canada. The present economy is one of hunting and trapping for meat and furs, and the exchange of the latter for foodstuffs and manufactured goods with some fishing, gardening, collecting and home production of equipment. In his section 'Technical Culture' Honigmann describes in considerable detail their combination of old and new techniques and materials.

The Kaska today live mainly in small mobile bands, gathering at the trading post in summer rather than as aboriginally in a lake fishing settlement. The population of the Kaska 'nation' is about 200; the 'tribe' with which Honigmann was especially concerned numbers 42, and he spent three months with a 'family settlement.' Temporary or permanent matrilocality is the rule, and there is frequent intermarriage with other Kaska 'tribes', neighbouring Indian groups and whites. Marriage seems to be unstable: illegitimacy, adoption, pre-marital sexual relations, separation and divorce are mentioned for many of his informants. Honigmann fails to give us an account of kinship relations within the 'tribe' or 'family settlement', and the transfer of trap lines, an important property and decisive factor in residence, is not made quite clear. Such omissions might be expected from the book's title, but a long 'Social Culture' section ('Organization', 'Interpersonal Relations' and 'Life Cycle') includes related matters. Furthermore, the personality materials at the end of the book suffer because relatives (e.g. uncle) are not clearly identified.

The section on 'Ideational Culture' is an account of Kaska beliefs

about and attitudes toward 'the supernatural, natural and social environment.' It includes material on such diverse topics as numbers, personality tests, beliefs about animals and ethical ideas. No reasons are given by the author for this extraordinary classification. Even as it stands it would be improved by the addition of information like the number of people questioned, their ages, sex, and the interest they took in the investigation.

Kaska 'ethos' is discussed by reference to the 'basic' or 'dominant motivations' of 'egocentricity', 'utilitarianism', 'deference', 'flexibility', 'dependence' and 'emotional isolation.' While the study of ethos is one of the more interesting anthropological developments, this is not a fully successful method: some 'motivations' are misnomers (e.g. 'deference' for avoidance of violence), each covers a number of motives and attitudes, and there is (as Honigmann admits) considerable overlapping among them. Honigmann says that he has defined the 'basic motivations' so as to be 'applicable only to the Kaska' (p. 26). A glance at the definitions (pp. 247-87) reveals that he has stated them in very general terms. Why should he believe that these apply only to the Kaska? Certainly the behaviour (or culture) of the Kaska is distinctive, but the 'basic motivations' listed operate among all people and do not in any way distinguish the Kaska. Again, some of these 'motivations' are conflicting, and an indication of relative stress on them in different individuals would be of interest; the five personality pictures he gives do not confirm all six 'motivations' as 'dominant' in all persons.

PAULA BROWN

**Handbook of Latin American Studies, No. 12, 1946.** Edited by F. Aguilar. Prepared by the Hispanic Foundation of the Library of Congress. Harvard Univ. Press (London: Cumberlege), 1949. Pp. x, 364. Price £3 8s.

The latest volume of this most useful guide to the literature in the fields of the humanities and social studies lists, with critical and informative notes, over 3,500 books and articles published in 1946. The book follows, in general, the arrangement of the previous volumes but there are two important differences. In accordance with the recommendations of the Advisory Board of the Handbook, the sections on Archives and Folklore have been omitted. Bibliographical material is included in the various subdivisions of the History section, and the main publications which would have been, in previous volumes, listed under Folklore are included under Ethnology, Music or other appropriate heading. The folklore material is inevitably more slight, but Ralph S. Boggs, who edited the Folklore section, is compiling an annual bibliography of Latin American folklore material, which is published in the *Southern Folklore Quarterly*. The second change is the omission from this edition of the sections on Labour and Social Welfare. The most important material published in 1946 will be included in the 13th Handbook. Each of the sixteen sections is preceded by a summary of the year's chief events and developments, written by the subject editor. To the non-specialist one of the most useful features of the book is that the great majority of books written in Spanish are reviewed in English. Miron Burgin, who edited the sixth to eleventh volumes, resigned in 1948, and this volume was produced under the editorship of Francisco Aguilar.

M. M. KERTON

**The Florida Indian and his Neighbours.** Edited by John W. Griffin. Winter Park, Florida (Inter-American Center, Rollins College), 1949. Pp. 168. Price \$1.

This series of essays is concerned mainly with the archaeological problems of Florida and with the possible early relationships between Florida and other parts of the Americas. John M. Goggin's 'Cultural Traditions in Florida Prehistory' and John W. Griffin's 'The Historic Archaeology of Florida' summarize archaeological research within the State. Charles H. Fairbanks then provides a broader regional picture of the south-eastern United States, defined as the area lying mainly between the Mississippi and the Atlantic coast south of the present states of Kentucky and Virginia. Succeeding essays by James B. Griffin, Gordon R. Willey and Irving Rouse discuss the possible cultural relationships of this area with Meso-America (mainly Mexico), South America and the West Indies.



respectively. Meso-America would seem to have exerted the strongest influence, seen particularly in the Mississippi period of the south-eastern United States with the development of the pyramidal mound as a substructure for important buildings. Willey agrees that basic agricultural technology probably came into North America from Mexico and Central America, but thinks that certain traits (e.g. platform bed, wooden stool, cane-slat and hide shields, use of litters for chiefs) may derive from the Circum-Caribbean cultures of South America. Rouse is of the opinion that on present evidence relations with the West Indies were not close. They were probably strongest at the pre-agricultural level and Rouse draws attention to the possibility that the Ciboney may have migrated from Florida to the West Indies. In a summary of these essays,

Willey agrees with the main conclusions, but rightly stresses that it is not known whether the influences discussed were the result of diffusion of ideas, of diffusion of goods by trade, or of actual migrations of peoples. He draws attention, also, to the basic difficulty that, whilst the recognition of changes in material culture within specific regions has helped to clarify the picture in some respects, it is still dangerous to overstress similarities in different regions as evidence of relationships. There is still a great need for chronological evidence and many of the problems associated with the agricultural groups of the south-eastern United States remain unsolved. All interested in these problems will, however, welcome this series of essays and appreciate the attempt made in them to summarize and define the present state of our knowledge.

S. J. JONES

## ASIA

**The People of Ceylon.** By N. D. Wijesekera. Colombo (Gunaseena), 1949. Pp. xix, 239, 25 plates

**II3** This book no doubt fills a long-felt desideratum, since it gathers together in one small volume a multitude of data otherwise to be found only in widely scattered publications. Moreover, it incorporates a liberal sprinkling of original observations made by the author during his association with the late J. R. de la H. Maret of the Ethnographical Survey of Ceylon, the results of which remain so far unpublished. Unfortunately the title of the book is misleading, for little or no information is given about any community in Ceylon other than the Sinhalese. A few discursive remarks alone are made upon the Veddahs in chapter III, where the question of racial composition is discussed, with further references to their earlier distribution in the island in the following chapter. From chapter V onwards till the last chapter (XXV) the social anthropology of the Sinhalese is the sole subject for treatment, but this, it must in fairness be stated, is treated adequately and exhaustively, from every conceivable angle.

The photographic illustrations are well chosen, and in the main well reproduced; but some of the other illustrations are of little or no merit. Charts such as those depicting extinct types of men and modern racial types compared would appear to have been photographed from a museum chart and suffer so much from over-reduction as to be quite meaningless. The book is well printed on good paper, but there are numerous typographical errors (e.g. *leaches* for *leeches* twice on p. 54) besides many 'Ceylonisms'—common local grammatical errors in English. There is an explanatory foreword by Julius de Lanerolle; but the inclusion of a personal tirade against the Museum authorities in the author's introduction, deserved though it may be, is somewhat unfortunate in a work such as this.

W. C. OSMAN HILL

**The Dove and the Leopard: More Uraon Poetry.** By W. G. Archer. Calcutta (Orient Longmans), 1948. Pp. xx, 175. Price 15s.

**II4** With his book *The Blue Grove* published in 1940 W. G. Archer put himself into the first line of the interpreters of Eastern folk poetry, and as joint editor of *Man in India* from 1942 to 1948 he has done much to acquaint anthropologists with representative specimens of Indian oral literature. *The Dove and the Leopard* is a sequel to his earlier work and contains 408 poems and 140 riddles rendered with the charm, simplicity and unflinching sense of balance which characterize all of Archer's translations. Comments on the social content and symbolism of the poems are fewer than in *The Blue Grove*, but in a weighty preface the author stresses the general relevance of the poems to anthropology. 'If a tribe is regarded as possessing in some degree its own pattern of culture,' he writes, 'its poetry and riddles are the ways in which that pattern is most clearly revealed. . . . The value of tribal poetry to anthropology is that it is the most sensitive instrument we have for diagnosing tribal differences.' He then compares Uraon poetry to the Baiga poetry recorded by Elwin, and demonstrates the basic differences between the two in regard to style and language symbolism as well as to the tribal attitudes to certain universal aspects of human life.

This comparative approach to folk poetry has great potentialities. Just as a comparative study of the plastic arts of primitive peoples

enables us to discern affinities, stylistic influences due to contact or migration and fundamental differences in artistic conceptions, so an analysis of their poetry may indicate connexions and groupings no longer apparent in spheres of culture more susceptible to extraneous influences. But the student of poetry is confronted with one problem absent from the study of plastic art. He has to take into account differences of language, and the co-existence of three major language groups—Aryan, Dravidian and Munda—in Middle India poses a number of complex problems. Foremost is the question whether poetic style cuts across the frontiers of language and peoples, so to say, an identity independent of its manifestation in the vehicle of a specific tongue. This question gains special relevance when tribal and linguistic boundaries do not coincide. In the Central Provinces, for instance, there are Gonds who speak one of the Dravidian Gondi dialects and Gonds who, having lost every vestige of their old tribal tongue, know no other language but the Hindi dialect current among the Hindu populations sharing the same habitat. If it could be shown that certain distinctive features of Gondi poetry occur also in the songs of those Gonds who have adopted Hindi as their medium of artistic expression, one could speak of a 'resistance of poetic style to linguistic changes,' but comparative studies have not yet reached a stage where such an assumption could be either proved or disproved.

Archer does not deal with this problem explicitly, but in his appraisal of Baiga poetry he clearly implies that Baiga songs, though composed in Hindi, yet reflect the tribe's traditional attitude to life and are thus indicative of the Baiga 'national' character. Although Baiga poetry would seem to evince certain features which distinguish it from the folk poetry of other peasant populations speaking Chhattisgarhi Hindi, there can be no doubt that the adoption of Hindi in place of a now submerged non-Aryan tribal tongue must have had a profound influence on the development of Baiga poetry.

The Uraon dialect in which the poems collected in *The Dove and the Leopard* were composed is a Dravidian tongue believed to belong to the same 'intermediate' group of Dravidian languages of which Gondi forms a part. Yet Uraon songs differ considerably from Gondi poetry, and it may well be that in style and content they stand closer to the poetry of the Uraons' Munda-speaking neighbours than to that of the Dravidian-speaking Gonds of the Central Provinces, Bastar and Hyderabad. A characteristic feature of many Uraon songs, and one which distinguishes them from Baiga songs, is a symbolism for which the text of the song does not provide an overt explanation, but which can be understood only through the context in which the song is sung. In the poetry of the Gondi-speaking Raj Gonds there is much less of this unexplained symbolism, and the short dance songs, which superficially resemble some of the Uraon songs, are not characterized by a predominantly symbolic content. The Uraons, on the other hand, appear to lack the lengthy narrative dance songs which constitute so important an element in Raj Gond poetry. Here then are obvious differences in the poetic styles of two Dravidian-speaking tribes, and analytic comparison of the more technical aspects of verse form and metre might reveal many more.

Indeed the need of the hour is a systematic analysis of poetic forms and styles, such as could provide us with a scheme of classifica-



tion, and thus prepare the ground for valid comparison. W. G. Archer, who has both the poetic feeling and a good deal of the indispensable linguistic equipment, would seem well qualified to undertake such a classification of Middle Indian folk poetry, and we may hope that at some future date he will undertake a systematic treatment of the many problems raised by implication in *The Dove and the Leopard*.

C. VON FÜRER-HAIMENDORF

**The Reddis of the Bison Hills: A Study in Acculturation.** By

**II5** Christoph von Fürer-Haimendorf with Elizabeth von Fürer-Haimendorf. (*The Aboriginal Tribes of Hyderabad, Vol. II*). London (Macmillan), 1945. Pp. xvii, 373, 84 illus., 5 maps

This survey records part of the work carried out between 1940 and 1943 by Dr. Fürer-Haimendorf for the Hyderabad Government, whose new policy towards the tribal groups of that State was largely influenced by his recommendations. It deals exclusively with the Hill Reddis, a partially detribalized group of shifting cultivators, about 4,000 strong, living in the hills on both sides of the Godavari gorge. Though they bear the name of a large caste of Hindu cultivators in the Madras Presidency there seem to be no structural ties between the two groups, though they may once have been fellow subjects of the Reddi kings. This might account for their designation, and possibly also the fact that the Hill Reddis are all Telugu-speaking. Lower-caste Hindu ideas and practices are spreading among them, as a result of the considerable cultural and economic penetration of the area made in the last hundred years, especially in the gorge itself, where timber contractors employ the Reddis as a source of cheap labour. As a result of the differing rate of absorption we may expect to see sub-castes developing, but hitherto, apparently, the spread of caste has served merely to increase the degree of tribal consciousness and unity.

**Wallfahrtsorte Europas.** By Rudolf Kriss. München (Hornung), 1950. Pp. 323

**II6** In short and lucid articles the author has recorded the historical development of some 120 European places of pilgrimage; and even those who cannot read German will feel richly rewarded when looking at the 200 photographs which are extremely well selected and reproduced.

Professor Kriss emphasizes the importance of the sacred images. Every type seems represented, from primitive folk art to the most refined work of art. The votive offerings as well as the miracle books reveal the requests of the pilgrims and their gratitude for the healing of diseases and deliverance from road accidents, fire, lightning and the horrors of warfare. Nor is the present forgotten for the past: the Blitz during the last war, release from concentration camps and escape from behind the Iron Curtain are mentioned. Some votive paintings illustrating old costumes and primitive medical practices are especially noteworthy. Photographs of processions show Austrians, Bavarians, Belgians, Bretons, Spaniards and Yugoslavs in their national costumes. The dates of some of the photographs obviously differ and an indication of the approximate year in which they were taken might have been helpful. (Some misprints should be eliminated: on p. 233 'Teller für Staub,' and on p. 317 'Votive und Weihgaben'). The discovery of the statue of St. Anne d'Auray certainly points to a popular cult existing before the seventeenth century.

In the articles on Eichstätt and Wilparting (Bavaria), and Rankweil (Austria), English, Scottish and Irish missionaries of the seventh and eighth centuries are mentioned. The reference to King William II taking an oath on the *volto santo* of Lucca should be noted by all admirers of Langford Church (Oxon). England is represented by a single article on Walsingham. An article on Holywell, the 'Lourdes of Wales,' with its unbroken tradition, would have been as welcome as a description of St. Brigid's Shrine near Dundalk; the latter would have revealed many common traits with Breton places of pilgrimage.

The concluding chapter on 'The relations between western pilgrimages and the history of civilization and folklore' forms a veritable climax in this excellent book. The author's analysis of the

From Fürer-Haimendorf's clear and readable description of the tribe the old Reddi social system can be reconstructed, still persisting under its overlays of alien practice. The main problems he has set himself however are practical ones rather than abstract, an elucidation of the problems that the process of acculturation presents to the administrator: The Reddis suffer on the one hand from the Forest Laws that strike at the basis of their traditional economy, and on the other from exploitation by members of the more advanced society into whose arms they are driven by the enforced restrictions on shifting cultivation. The remedy suggested approximates to that of Elwin. While absorption seems ultimately inevitable, the hardship implicit in it can be minimized if it is gradually brought about, and the Reddis given the chance to equip themselves to face it. A degree of protection has already been given by legislation passed since the submission of Fürer-Haimendorf's report, but this is only a time-winning measure. Positive action must come from within the tribe, either from trained Reddis or independent social workers such as the 'Swami of Parantapalli' mentioned in the book. This anonymous Brahmin, with a few assistants, has gained the confidence of the Reddis after long residence among them, and has successfully organized a Reddi timber co-operative, in open competition with Plains contractors. The best long-term policy, it is suggested, is the encouragement of this kind of thing, and the training of Reddis for forest-conservation, and their large-scale employment by Government for Forest labour, as entailing the minimum conflict with Reddi traditions and ways of life.

Indian anthropology has long been vexed by Gazetteers and Surveys. Detailed studies such as those of Dr. Fürer-Haimendorf are the real need, and we must hope for much more from his pen, and for many more fieldworkers of his ability.

R. L. ROOKSBY

## EUROPE

various influences affecting pilgrimages, of the parts played on the one hand by the Church and on the other by the people themselves, will have to be consulted by all students of the history of religion.

ELLEN ETTLINGER

**Gods and Heroes of the Celts.** By Marie-Louise Sjoestedt, trans. Myles Dillon. London (Methuen), 1949. Pp. 103. Price 8s. 6d.

**II7** This translation of *Dieux et Héros des Celtes*, which was published in Paris in 1940, is embellished with a preface and some additional footnotes and bibliographical references by the translator. In undertaking this edition, Professor Dillon has rendered great service to the cause of broadening the approach to early European studies, for this book by the late and much lamented Marie-Louise Sjoestedt is something new and important in the field of Celtic mythology.

The book finally disposes of the ideas so long propagated that the Celtic peoples possessed some kind of an organized pantheon after the received Classical manner. It is stimulating to find that the great corpus of pre-Christian 'sacred' tradition, preserved in Irish and Welsh literature and in the relics of philology and archaeology, is in fact capable of interpretation in terms appropriate to barbaric peoples locally organized, and divorced in their rural life from the departmentalizing tendencies of urban systems. The numerous deity names of both sexes can, as Mlle Sjoestedt demonstrated, be understood as tribal and regional variations of two basic concepts: the god of the tribe who is its ancestor, benefactor and protector, and the goddess of the natural region on whose goodwill depends the fertility of man, beast and vegetation. Any god may therefore be expected to possess many attributes, and the goddesses conform to a fertility pattern. Stories have survived in Irish tradition of the union of the tribal god and the territorial goddess, and this seems to have been one of the principal ritual events of the great autumnal festival of Samhain, when, as later recounters put it, 'all the fairy mounds in Ireland were open.' The goddesses also had aspects of destruction and carnage, glorying, but not participating, in the slaughter of the battlefield. It is not however desirable to isolate these characteristics any more than it is to try to distinguish between



the individual members of the triple deities of one name, who, occurring in both sexes, may recall some stage earlier than the fully developed anthromorphic deity. Zoomorphism is known amongst the Celtic deities of both sexes particularly in the name forms. The tribal god appears on the whole to have been envisaged as a crude being of great physical and sexual prowess, skilled in magic, but wielding primitive weapons such as the club. The Cerne Abbas Giant almost certainly depicts such a tribal god. A more advanced type is represented by the god Lug, particularly noted for his skill as a craftsman and a wielder of the spear and sling. It is possible that he is to be associated with the peoples of a particular culture province so far as Britain and Ireland are concerned.

The seasonal nature of Celtic religious observance is well seen in the organization of great seasonal festivals, of which there were four, the principal being Samhain, held after the harvest when the agricultural year died. It seems likely that all occasions of importance, such as the inauguration (*lit.* wedding feast) of kings, were confined to these gatherings, and the places where they were held were royal tombs or other sacred precincts, not the strongholds of the living. The evidence about actual rites and their illustrative myths is more elusive on account of their direct antagonism to Christianity. Human sacrifice was certainly not unknown, and it seems to me possible that a violent end was arranged for the kings of some peoples at least, when past their prime. The inauguration of a king through union with the local goddess is seen not only in the late-surviving Ulster example described by Giraldus Cambrensis, but in the stories about kings of Tara where a hag, fearlessly approached, turns into a beautiful girl, the assurance of an auspicious and bountiful reign.

The latter half of the book is devoted to a discussion of great value on the types of heroic society portrayed in the Irish *seála* or sagas. Other writers have attempted to show that the great story cycles, centred on Cuchulain and Finn, were mythological in character, but these views have generally been put forward by people acquainted only with the literary and philological aspects of the material and unequipped to assess its meaning. Mlle Sjoestedt distinguished two types of heroic character in ancient Irish society: the tribal hero and the extra-tribal or unattached hero. The tribal hero, typified in Cuchulain, embodied all the accomplishments to be desired in an aristocratic barbarian warrior. His virtues and

prowess are extolled from his childhood and upbringing amongst the boys at the court, through qualifying exploits to become a warrior with the privileges and duties thus acquired, to his winning of a superlative wife, and finally of his heroic death, brought about not by the superior arms of the enemy but by magic and the disregard of ritual sanctions with their inevitable penalty. The other kind of hero presents a more complex situation. It has long been recognized that martial personages of the type of Finn and the warrior bands known as *fiana* represented some kind of organization different from that of the Cuchulain type. Suggestions have been put forward that the *fiana* were bands of outcasts, plunderers, or mercenaries, but none of these descriptions agree with the evidence, which shows that members of the *fiana* had close relationships with settled communities, and could return to normal society. Mlle Sjoestedt considered that the *fiana* were organizations which provided for the man who through temperament or for other reasons found no place in the normal tribal warrior society. In this case Finn would typify the ideal leader of such a band of voluntary exiles, ranging over the waste places hunting and foraging in strange country. There are, however, seasonal, distributional and genealogical factors concerning the *fiana* and their named leaders which strongly suggest that they did not belong to the same population groups that supported the aristocratic chariot heroes such as Cuchulain. It seems to me highly probable that the *fiana* belonged to other, but still Celtic, peoples in Ireland, and that although the value of the term may have changed, they originally were the bands of the warrior age group in which all men served after initiatory tests; see *The Early Cultures of North-West Europe* (H. M. Chadwick Memorial Studies, ed. Fox and Dickens, 1950), pp. 177ff. There seems to be little doubt that by the time the Finn stories reached final development, the real nature of the *fiana* had long been lost owing to more settled conditions and the dynastic reorganization of the island.

Apart from this one difference, the present writer has nothing but admiring respect for this book, which notwithstanding its brevity is packed with information and written with great clarity and orderliness. It is tragic that a larger work by the same pen cannot be looked forward to, but in the existing book Marie-Louise Sjoestedt laid new foundations for the proper anthropological study of early Celtic institutions. T. G. E. POWELL

## CORRESPONDENCE

**The Dentition of the Australopithecine.** Cf. MAN, 1951, 37, 38, 41, 60, 61

**II8** SIR.—There is a danger that those who have followed the correspondence between Professor Le Gros Clark and Professor Zuckerman, concerning the dentition of the South African Fossil Anthropoids, may be led to think that the relationship of these Anthropoids to living Anthropoids and to man has to be determined solely on the evidence of the teeth. The chief importance of the discovery of the Australopithecine (or Dartians, as I ventured to name them) for students of human evolution is the fact that their pelvis, their limb bones and their skulls are modified for life on the ground, and that they held their bodies and walked or ran much as man does. To adapt an anthropoid for a human posture entailed such a multitude of structural alterations that it is almost inconceivable that the transformation could have happened twice—once for the human family, and once for the Australopithecine. We must assume, I think, that the human and Dartian postures are a common inheritance from a stock not yet discovered. The locomotor system links the Australopithecine very much closer to man than to any anthropoid known to us.

For more than fifty years I have been studying and comparing human and anthropoid teeth. If I had been shown detached first lower premolars, such as occur in the mandibles of the South African Anthropoids, I would have unhesitatingly ascribed them to a member of the human family, for only in human mandibles had I seen teeth with such characters. So, too, had I been shown the detached molar teeth, I would have said they were human,

for only in the human mouths have I seen molars with such a cusp pattern. In the shape and size of their canine teeth the Australopithecines are more akin to man than to any form of anthropoid ape.

Yet I must admit that if I had been shown the outline of a South African anthropoid skull, in full profile or in full face, and asked to say what genus of Primate it represented, I would unhesitatingly have replied 'anthropoid.' The Australopithecines are a strange mixture of ape and man.

In 1920, my friend the late Dr. Carveth Read (then lecturer on Comparative Psychology in University College, London) published a book on *The Origin of Man and of his Superstitions*. His chief thesis was that, to account for human psychology, it was necessary to postulate a stage in the evolution of man in which our earliest ancestors were grouped in hunting packs. I, in common with others, looked askance at his theory because in 1920 anthropoids, as far as we knew them then, were the last beasts we could suspect of forming hunting packs. In 1925, Raymond Dart changed all that and, by the discovery of the Australopithecine, provided just the communities that Carveth Read had dreamt of.

Downe, Farnborough, Kent

ARTHUR KEITH

**The Framework of Prehistory.** Cf. MAN, 1951, 64

**II9** SIR.—Not only archaeologists but all anthropologists should welcome Dr. Daniel's attempt to provoke a scientific discussion of the framework of prehistory, for a correct understanding of it is indispensable for an examination of questions



which even social anthropologists may legitimately put to prehistoric archaeologists. No one appreciates better than I the confusions due to traditional archaeological terminology, and I can but be flattered by his commendation of departures from current usage that I have ventured to initiate. But in his criticisms thereof he fails to explain to anthropologists the precise nature of the problems confronting the archaeologist. He complains that my 'periods are still both cultural and chronological'; he might legitimately have gone on to point out that just for that reason my system was in effect applicable only to the 'chalk lands of Southern England.' My Period I was in fact defined by the culture characterized by the simplest Neolithic A1 pottery; VI by the Deverel-Rimbury culture; VII by Iron Age A; and IX by the Belgæ. But as none of these cultures can be detected in Scotland, these 'periods' are not represented in the Scottish sequence. That is precisely because they are archaeological periods and such, by their nature, are, as Daniel says, 'in reality typological and cultural divisions.' In other words, they are not divisions of abstract time, but divisions of archaeological material serialised not in space but in a fourth dimension. Archaeologists, like geologists, can observe series in the third dimension—vertically superimposed strata. They then transpose them into a fourth dimension—temporal successions.

Let us accept the metaphysical assumption that this fourth dimension coincides with the time of Newtonian physics and should be measured by repetitions of self-identical events, merely noting that this is an assumption. Geological and archaeological strata are not self-identical and the terms of our series are distinguished not by position alone but by qualitative content. Furthermore, the position in the series of a term is itself determined by its content. Each sequence has turned out to disclose an order of a higher kind and become a progression. If only to discover this order, geologists and archaeologists have had to treat their fourth dimension as equivalent to clock time and to assume that their periods are measurable by repetitive events. But being at present unable to apply to their successive divisions or periods the convenient chronometer provided by the earth's revolutions round the sun, geologists distinguish the divisions of their sequence by names. The major divisions have been given descriptive appellations; Proterozoic, Mesozoic, Eocene, Pliocene then indicate the position of the period thus designated in the progression. For the subdivisions conventional geographical designations were generally retained. Prehistorians, suffering from the same disability, adopted a similar system of nomenclature. Palæolithic, Neolithic, Bronze Age and Iron Age do indicate stages in a technological progression. De Mortillet's 'Chellean,' 'Acheulian,' etc., deliberately imitate 'Silurian' and 'Devonian.' The archaeologist, however, is worse off than the geologist.

Of course in no two sections are the observed sequences quite identical in content, but geologists can ignore more of these differences than can archaeologists. In both disciplines the major terms are all homotaxial: they occupy the same positions in all series. The geologists' homotaxial terms are also systadial, occupying the same position in the palæontological hierarchy. And so, but in the same limited sense, do the archaeological 'ages.' The geologists have agreed to treat homotaxial and systadial terms as contemporary, i.e. as divisions of abstract clock time, even though they cannot yet be measured by an independent chronometrical scale. Prehistorians cannot be content with such a usage. They want to know, for example, whether the Bronze Age began earlier on the Nile than in Britain. They therefore seek to fit their local periods into the time scale measured by some independent and more satisfactory chronometer, whether it be movements of glaciers, changes in climate, disintegration of unstable atoms, or revolutions of the earth round the sun. The current usage of the term 'palæolithic' as equivalent to 'Pleistocene' conforms to this ideal, but conflicts with the stadal usage of the remaining major divisions and will certainly have to be amended. But in general the retention of the descriptive names has definite advantages, provided that it be recognized that they do not denote independent divisions of abstract time. 'Neolithic' or 'Iron Age' at once indicates the position of the local segment thus designated in a universally valid sequence.

Thirdly, the type fossils used by archaeologists to define and, in

fact, to constitute their divisions and subdivisions are not as nearly universal or evenly distributed as the genera of mollusca or mosses. They are determined by social traditions which change not only with time, but with the historical circumstances of the particular society that creates or approves them. And the latter differences are just as significant for prehistory as the former. Yet they are used to define both cultures and archaeological periods, while by their very nature they are confined to the society that initiated or adopted them, which is of course a spatially limited unit, and to such other societies as were in fairly close communication with the first-named. In practice the interchange of movable type fossils between distinct societies makes possible the correlation in time of such cultures or societies within the wider, but still always limited, 'province.' The type fossils then may be said to define an archaeological period within the geographical area of their regular distribution—the culture province. It may indeed turn out that even within such a province not all societies were at the same stage. In Denmark savage hunter-fishers of the mesolithic Ertebølle culture were living side by side with barbarian farmers building *dysser*. Nonetheless, no serious confusion should follow from applying to the period the stadal designation appropriate to the most advanced society in the province, provided that both the chronological use be emphasized by inserting the word 'period' or 'age' and its spatial limitations by a geographical determinant. To speak of an 'Ertebølle Neolithic boomerang' would be confusing. 'An Ertebølle boomerang of Danish Neolithic age' is unambiguous and no clumsier than 'of Danish Period N,' but a little more informative. Since, then, archaeological divisions are by their very nature essentially both cultural and local, it seems in principle desirable to give recognition to this fact by the insertion of cultural and geographical terms in the designation of the division.

When possible, for chronological purposes, i.e. for instituting comparisons outside a single cultural province, non-archaeological terms should be used, whether centuries in our era or climatological designations like Sub-Boreal and Sub-Atlantic. It must be insisted that cultures define only divisions of a local sequence. They should not therefore be used for divisions of any wider frame. Fortunately, however, actual interchanges of type fossils or close imitations of such in spatially adjacent and interrelated provinces do render possible correlations between the divisions in distinct sequences. But such correlations have still only a spatially limited validity. They permit several local sequences to be fitted into a wider frame of reference in the archaeological fourth dimension, but the frame's applicability is none the less spatially restricted. The problem which Dr. Daniel raises is therefore that of the nomenclature of these super-provincial frames of reference. It is clear that the nomenclature adopted must leave the non-specialist in no doubt as to the limitations of the frame and should not blind the student of archaeology to the fact that one of his main problems is the correlation of the terms between the established local sequences and the extension of such correlations over an ever wider area, an extension which will ideally lead to an absolute chronology based upon the historical records of Egypt, Mesopotamia, China, Greece and Rome.

I am always afraid lest a series of periods distinguished by consecutive ordinals may give a false impression of the incompleteness of the sequence and even suggest that the periods thus designated are equal divisions of abstract time. Perhaps after all it will turn out to be better to denote the divisions by the very cultural phenomena that make possible the correlation of the several periods, and we shall have hideous but accurately named divisions—Segmented Bead, Velatic Cup, La Tène I Brooch, etc., phases!

V. GORDON CHILDE

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**Social Anthropology: Past and Present.** Cf. MAN, 1950, 198, 254 and 271 and 1951, 33-35, 62 and 78

**120** STR.—It seems, alas, that Professor Evans-Pritchard, whose achievements in the field of ethnography are undoubtedly of lasting value, shares the prejudice against the scientific study of societies which is unfortunately very common in the British universities, particularly in Oxford. The argument that no valid generalizations can be found in their fields has been repeatedly



used against all budding sciences. Had it been heeded there would be today no physics, no chemistry, no biology—indeed, no science whatsoever. Even if no regularities in the life of societies had been discovered so far, that would not be a proof of the futility of the search for them. Every science must begin sometime. But the situation is not quite so bad: some sociological generalizations have been established, although their number is still very small. It is known, for instance, that the forms of power tend to coalesce, that is to say, that those who have the political power tend to acquire the economic, and *vice versa*; that extensive polygyny goes with great economic inequalities; that war promotes autocracy; that overpopulation tends to produce war. I cannot multiply the examples, but I wish to point out that in kinship organization—the field with which anthropologists traditionally occupy themselves—quite a number of regularities have been found by Radcliffe-Brown, Lévi-Strauss and G. P. Murdock.

I must point out to the devotees of pure description and 'historians *historisants*' that description is possible only with the aid of words which are general concepts. All description, therefore, presupposes recurrence of elements. Something that is completely unique is also indescribable. Moreover, description means selection of data from infinite reality, and such selection presupposes some kind of ideas as to what is important; ideas, that is to say, about what can cause what. If in an ethnographic study we get the details concerning the constitutional rights of the king, and not the description of the shape of his toes, it means that the writer holds theoretical views according to which the monarch can cause more things to happen in virtue of his constitutional rights than in virtue of the shape of his toes. Finally, unless we possess some generalizations all descriptive knowledge is useless. Usefulness means prediction, even if within very broad limits, and no amount of descriptive data permits extrapolation without the aid of generalizations. If there were no regularities in the life of societies, then Professor Evans-Pritchard would be no more qualified to give advice to somebody who will have to deal with the Senussi than my illiterate servant who never heard of them—because in that case anything might happen. Tomorrow they might all become Jesuit monks and ask the nearest relative of Mussolini to be their abbot. We know that this will not happen—but how do we know it? In the same way as we know that from tomorrow water will not start running upwards—because such things have never happened before! We reason about the case in hand on the basis of our knowledge of other cases. To be consistent, those who maintain that the search for regularities in the life of societies is futile ought to admit that even though ethnography and history may be pleasant hobbies, they are as useless for understanding societies as is the knowledge of how to play canasta.

In reality all historians, ethnographers, social reformers, politicians—everybody in fact—have a stock of rule-of-thumb sociological generalizations, even if they do not realize it, without which they could not act. The task of the social sciences is to make these generalizations more explicit, to test and sift them, and to discover new ones.

How theoretical assumptions underlie all action can be seen from the example of the manifesto on race sponsored by U.N.E.S.C.O. Here the assumption is that conflicts between 'races' are due to the intellectual error of believing in their biological inequality. Nothing could be further from the truth. These beliefs are mere justifications of claims to wealth, power and prestige: they are epiphenomena of group struggles where the frontiers between the groups correspond to the distribution of racial traits. The propaganda about presumed biological equality of races must, therefore, be even less successful in stamping out conflicts between races than were the teachings of the universal religions. Before we can do anything about these conflicts we must know their causes.

I do not think that the hostility to the search for regularities can threaten the progress of social anthropology, which is now quite well established. But it has arrested the progress of sociology in Britain. It has been finally accepted that primitive societies can be studied scientifically; but the complex societies remain taboo. The result is that in the field of sociology the British have made in this century fewer contributions than any other nation in Europe, except perhaps the smallest ones. In the number of sociological

works published between the two wars Britain is far behind even countries so severely handicapped as Poland and Czechoslovakia. In comparison with France, Germany and the United States (even though there quality does not correspond to quantity) Britain is unbelievably backward. Only Russia falls behind Britain. It appears, therefore, that the blind conservatism of the British universities and the obscurantist belief that 'it cannot be done' have been almost as successful in stifling sociology in Britain as were the Stalinist police in the Soviet empire.

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Grahamstown, South Africa

**Malinowski's Views on Magic and Science.** Cf. MAN, 1951, 36

**121** SIR,—Malinowski had no consistent theory of magic. On p. 56 of his *Magic, Science and Religion* he speaks of 'the conviction that only by an absolutely unmodified immaculate transmission does magic retain its efficiency. The slightest alteration from the original pattern would be fatal.' Three pages on he says the exact opposite, namely that 'each type of magic, born of its own situation and the emotional tension thereof, is due to the spontaneous flow of ideas and the spontaneous reaction of man.' He tells us, however, that in the Trobriands open-sea fishing, which is dangerous and uncertain, is accompanied by much magical ritual, while lagoon fishing, which is safe and certain, has no such accompaniment, and his general attitude is that magic comes in where knowledge and confidence fail.

There is no doubt some truth in this, but that it is not the whole story a study of metallurgy shows. Iron-working has been for many peoples ancient and modern a magic craft, not to be practised without the observance of many rites and taboos. Except perhaps to the outsider there is, however, nothing mysterious about it. As it is carried on, for example, in Central Africa, anyone who masters the laborious but quite simple technique can be sure of his results. Hocart cites Knox as saying that formerly the Sinhalese smith left all the work to his customer, merely holding the iron and perhaps giving a finishing touch. The work was so simple that anyone could do it, but the ritual assistance of the smith was nevertheless considered necessary. It is difficult to believe that such observances ever had anything spontaneous about them.

Usk, Monmouthshire

RAGLAN

**Race.** Cf. MAN, 1951, 95, 96

**122** SIR,—I am afraid that Cedric Dover has misunderstood my allusion to 'race' as a 'group concept.' By this I meant simply that the term 'race' is essentially a concept relating to a group of people considered as a group. The point is methodological, not sociological. Needless to say, I am as deeply convinced as Dover of the biological meaning of 'race' and thought that I had made this clear in my advocacy of the term.

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**Corbelled Stone Huts in Southern Africa.** Cf. MAN, 1951, 97

**123** SIR,—With reference to my article in the April issue, may I add the following further references to published accounts of corbelled-hut settlements in Southern Africa to those given in my note 1?

(d) T. R. Jones, 'Prehistoric Stone Structures in the Magaliesberg Valley, Transvaal,' *S. Afr. J. Sci.*, Vol. XXXII (1935), pp. 528-36.

(e) R. A. Pullen, 'Remains from Stone-Hut Settlements in the Frankfort District, O.F.S.' *S. Afr. J. Sci.*, Vol. XXXVIII (1942), pp. 334-44.

(f) F. Daubenton, 'A Preliminary Report on Stone Structures near Steynsrust, O.F.S.' *S. Afr. J. Sci.*, Vol. XXXV (1938), pp. 364-70.

(g) P. W. Laidler, 'The Archaeology of Certain Prehistoric Settlements in the Heilbron Area,' *Trans. Roy. Soc. S. Afr.*, Vol. XXIII (1935), pp. 23-70.

May I also record my thanks to the Leverhulme Research Fellowships Committee, a grant from which enabled me to carry out this study?

Masereng, Basutoland

JAMES WALTON









(a) WOODEN FRIEZE CARVED IN VERY DEEP RELIEF IN THE OWO STYLE: LENGTH 17 FEET  
By courtesy of the Trustees of the British Museum and The Times



(b) HEAD OF A RAM IN THE COLLECTION OF  
M. COCKIN, ESQ.: HEIGHT 19½ INCHES  
Photograph: British Museum



(c) HEAD OF A MAN WITH RAM'S HORNS :  
HEIGHT 16½ INCHES  
Photograph by William Fagg (Owo, 1950)



(d) IVORY FIGURE IN THE  
BRITISH MUSEUM :  
HEIGHT 7½ INCHES



(e) CEREMONIAL SWORD OF CARVED IVORY IN THE BRITISH MUSEUM:  
LENGTH 18 INCHES

Photographs by courtesy of the Trustees of the British Museum

# THE ART STYLE OF OWO



# TRIBAL SCULPTURE AND THE FESTIVAL OF BRITAIN\*

by

WILLIAM FAGG

*British Museum*

**124** In June and July, 1949, as readers of *MAN* will remember (1949, 79 and 145), the Royal Anthropological Institute held an exhibition in its lecture room under the title 'Traditional Art of the British Colonies.' The immediate purpose was to make a contribution (with the generous assistance of the Arts Council of Great Britain) to the Colonial Month organized by the Colonial Office, but it was also intended to be a 'pilot' exhibition, demonstrating in a small way the possibilities which could be realized in larger exhibitions of tribal art arranged with a proper balance of æsthetic and ethnological considerations, such as this country has had too little opportunity of seeing.<sup>1</sup>

Part of this second purpose was quickly achieved when, later in the same year, the then Secretary of State for the Colonies, Mr. Creech-Jones, took the Institute's exhibition as the model for a much larger show of 'Traditional Art from the Colonies' to be held during the Festival of Britain. Fellows of the Royal Anthropological Institute have taken a leading part in consultative capacities in the planning of this exhibition, which was to be visited by H.M. The King on Empire Day, 24 May, and opened to the public on the following day; it is to remain open, in the Art Gallery of the Imperial Institute, until 30 September.

As in the R.A.I. exhibition, the selection has been made on the basis of æsthetic merit, an effort being made to represent all territories, administered by the Colonial Office, in which indigenous art of the first class has been found. (Many Colonies which have no tradition of representational sculpture are represented in the craft section of the exhibition adjoining the main display of sculpture.) A number of Colonial Governments have sent objects on loan to the exhibition, and all those represented have borne a share of the cost. The Chairman of the Committee which advised the Colonial Office on the scope of the exhibition was the Rt. Hon. Lord Listowel, P.C., the selection of objects was made by Mr. H. J. Braunholtz, Mr. Webster Plass, Mr. Leon Underwood and myself, and the exhibition has been organized by Mr. Basil Taylor. Responsibility for the mounting of the exhibition has been taken by the Central Office of Information. A detailed catalogue of the sculpture section has been prepared, including such documentation of specimens as is available, and there is also an illustrated handbook containing 36 plates with an introduction.

His Majesty has graciously lent one of the magnificent pair of leopards carved in ivory from the Oba's Palace at Benin, which were presented to Queen Victoria and placed on loan in the British Museum by King George V. For-

\* With Plate F and a text figure

tunately, the very stringent conditions which govern the loan of specimens by the Trustees of the British Museum could all be complied with in the case of this exhibition, and a large number of fine pieces have accordingly been lent from the Department of Ethnography. Other museums which have made notable contributions are the Pitt-Rivers Museum at Farnham, Dorset, the Cambridge University Museum of Archaeology and Ethnology, the Maidstone Museum, the Royal Scottish Museum and the Pitt Rivers Museum, Oxford. Private collections represented include those of Mrs. I. M. Beasley, Mr. R. P. Bedford, Mr. Maurice Cockin, Mr. Kenneth Murray, Mr. Webster Plass, M. Charles Ratton (Paris), Mr. Robert Sainsbury, Mrs. B. Z. Seligman and Mr. Leon Underwood, and the Fuller Collection; special mention should also be made of the Oni of Ife, the Olowo of Owo, Chief Oludasa of Owo, and the Benin Native Authority, whose agreement to lend some of their most valuable pieces has greatly increased the importance of the whole exhibition.

The exhibition is, indeed, likely to be recognized as one of the most important displays of tribal art ever held, in this country or elsewhere, and the Colonial Office is to be warmly congratulated upon presenting this unusual opportunity to artists, ethnologists and the general public to appreciate in the most favourable conditions the works of art of the tribal masters of West Africa and the Pacific Colonies. At the same time it must be made clear that we look beyond this show to a far more comprehensive exhibition of tribal art; for this one is by definition confined within certain modern and arbitrary political frontiers which bear little or no relation to the great complex of æsthetic and cultural traditions which forms the African past. The rooms of the Royal Academy may well be the most appropriate place, in a few years' time, for an international exhibition of African sculpture in all the immense variety of its phases and distinct styles.

## *Summary of the Exhibits*

*Nigeria.* Considerably more than half the exhibits are (as in the R.A.I. show) from Nigeria, the largest, most populous and most prolific in art of all the British Colonies. The Oni of Ife has lent a generous selection of the famous bronze, or rather brass, heads dug up there in 1938-39 (including the two recently returned to the Oni by Professor W. R. Bascom, after 12 years in the United States), and the finest of the terra-cottas; an unusual opportunity is thus offered for the study at first hand of this extraordinary efflorescence of quasi-classical naturalism. One of the finest and most varied displays of Benin sculpture in bronze and ivory ever gathered together will be on view.



The European public will be introduced to the magnificent antiquities of Owo for the first time; a more detailed account of these follows in the final section of this article. Many sub-styles of the Yoruba will be represented, as will the Ibo and other tribes of the South-East. From the North come many fine specimens of the arts of the pagan (*i.e.* non-Moslem) tribes, including some of the extraordinary abstractions used as dance headdresses by the Ham (Jaba) tribe; these had been kept secret from Europeans until 18 months ago, and specimens were collected specially for this exhibition. Three fine terra-cottas from the 2000-year-old Nok Culture—two of them excavated in the last few months—are included. From the grasslands of the British Cameroons, besides some of the boldly conceived human and animal masks, there is one of the four life-size naturalistic figures collected by the Germans from the palace of the Fon of Bèkôm.

*Gold Coast.* Ashanti is represented not only by fine bronze vessels, gold ornaments and brass goldweights, but by some of the few known pieces of traditional figure sculpture. But this section is specially noteworthy for the fine masks from the Northern Territories lent by Mr. Cockin from the collection made by the late Sir Cecil Armitage; these show that certain Gold Coast tribes can readily hold their own with their better-known Ivory Coast counterparts.

*Sierra Leone.* Both the woodcarvings and the older steatite rice-cult figures are shown in greater variety than is commonly credited to them.

*East Africa.* Examples are shown of what are virtually the only occurrences of representational art east of the Great Lakes—the masks of the Makonde of Tanganyika and the bowls decorated with carved animals of the Barotse of Northern Rhodesia.

*The Pacific Colonies.* There are figures, masks and house-boards from Sarawak, and a number of fine early pieces collected by J. L. Brenchley in the Solomons in the mid-nineteenth century (lent by the Maidstone Museum), besides some outstanding pieces from the New Hebrides, Fiji and Tonga.

### *The Art Style of Owo*

The art of Owo is singled out for special treatment here because this is the first occasion, so far as I know, on which pieces in this style have been exhibited, at least in Europe, as a group and identified by their place of origin, and because it may well prove to have an importance in the art history of West Africa comparable with that of Benin and of Ife.

Owo is a fairly large town some 60 miles north of Benin (and not to be confused with the small village of Owo, west of Lagos, mentioned in my article in *MAN*, 1950, 234). Its people are Yoruba-speakers of the Ekiti sub-tribe and their hereditary ruler or Oba is known as the Olowo of Owo. Their material culture is, at least on a short acquaintance, far more Bini than Yoruba in character: the Olowo's cap and ceremonial garments are made from the same red coral beads as those of the Oba of Benin, and

not from the multi-coloured trade beads from which the crowns of other Yoruba chiefs are almost universally made; his page carries the scimitar-like *ada* sword, and he has in his palace a large collection of the 'fish-slice' dancing swords (*ebe* or *ebere*), with loop handles at right angles to the plane of the openwork blade; the parallel horizontal grooves which embellish the well kept walls of the great houses of Benin reappear here, though narrower and more closely set, on the inner walls of a number of buildings; and the art forms to be found in the town mostly have their clearly recognizable counterparts among the well-known products of Benin. Accordingly to Bini tradition and some European writers Owo was at one time under the suzerainty and protection of the Obas of Benin, but this the present Olowo, a cultured and intelligent man, strenuously denies. That there has at some if not all times been a strong cultural connexion is, however, perfectly clear.

Owo art takes the form for the most part of carving in wood and ivory, whereas most of the finer Bini work is in bronze; there are, however, a number of minor works in bronze, some of excellent quality, in the possession of various chiefs at Owo, and one small mask of the type worn by chiefs on the left hip at the point where the skirt is tied will be seen in the exhibition.

Among woodcarvings we may note first the presence of architectural sculpture in wood on a scale that does not seem to have been attempted at Benin. When I visited Owo, with Mr. Kenneth Murray, Surveyor of Antiquities, Nigeria, in March, 1950, I was shown a fine example of this kind of work over the great ancestral altar in the house—one of the best-preserved houses in the ancient style in Yorubaland—of Chief Oludasa, a lesser chief of the town. Both the heavy wooden ceiling and the rear wall of the altar are finely carved in deep relief with human and animal forms, arranged and designed with great taste and sense of proportion so that the figures stand out firmly in the gloom under the overhanging roof. Fortunately, an example of this art form has recently been acquired by the British Museum from the Wellcome Historical Medical Museum, though it was at first assumed to be Benin work, and this has been lent for the present exhibition. This piece, carved in three separate sections, is illustrated in Plate Fa; its total length is 17 feet 6 inches and the average thickness is between seven and eight inches. Though many of the subjects appearing in this frieze are found also at Benin, the treatment of them is distinctly different: for example, the two projections from the cap of the Olowo, whom we may assume to be represented in the centre, have their counterpart at Benin, but are there always shown upright, perpendicular to the ground (although in actual wear by the Oba these flaps or wings of coral beadwork do sag at an angle of about 45°), whereas in Owo art they always appear as here and in Plate Fe, as well as in the bronze pommels, cast in the form of the head of an Olowo, of a number of steel rapier-like swords in the possession of the present Olowo; also, the heads of the crocodiles at each end of the frieze are shown in the same way as on certain ivory carvings known to have been



made at Owo, and not in the Benin manner. Here, as in all Owo carving, the excessive preoccupation of the Bini with minor decorative detail has been successfully avoided in favour of the Yoruba technique of exaggerating the size of small objects or parts of figures to produce a more even distribution of sculptural interest. The most striking thing to note about this carving is, however, the fact that the subjects, although not carved in the round, stand out (like type from the type body) by about three inches, without any undercutting, or about twice what the thickness of the figures would be if carved in the round. The effect of this is to throw the deeply excavated background into deep shadow so that the figures seem to project themselves in a most effective way.

The confident boldness of line and volume which distinguishes Owo sculpture from the rather stolid and pedestrian woodcarving of Benin is well seen in the magnificent ram's head in Plate Fb, one of a number of Owo pieces lent by Mr. Maurice Cockin, to whom they were given by the then Olowo about 30 or 40 years ago. The same quality is seen in the human head with ram's horns (Plate Fc), one of three which I photographed in the house of Chief Oludasa; one of these has been lent by him for the exhibition. Both types of head are used in the chiefly ancestor cult, performing the same function as the bronze heads found on the ancestral altars of the Oba of Benin in



FIG. 1. A CEREMONIAL DRINKING CUP FROM THE BUSHONGO OF THE KASAI, BELGIAN CONGO

*By courtesy of the Trustees of the British Museum*

1897, and the wooden heads still to be seen on the altars of some of the lesser Bini chiefs; that is as a kind of receptacle into which the spirit of the ancestor can be summoned during rites in his honour. (Like the wooden heads at Benin, these Owo heads are all provided at the back with a vertical socket for a short stick, which may be intended to be rattled when summoning the spirit, or merely as a support for a small elephant tusk.)

It may be permissible to cite here (as fig. 1) an example of the only other established form known to me in African art in which a human head is provided with the horns of a

ram. This is the well-known type of ceremonial wooden cup found among the Bushongo of the region between the Kasai and Sankuru rivers in the Belgian Congo, and particularly among the Bambala, or royal, sub-tribe at Mushenge. The precise purpose of these cups is somewhat obscure, but in some instances the horned head surmounts a female body. I do not wish here to claim either convergence or diffusion as explanation of the similarity, but some causal connexion is not inconceivable, since, according to the traditions noted by Torday, the ancestors of the Bushongo travelled westwards to the neighbourhood of Lake Chad before continuing their journey southwards to the central Congo, and on the other hand there is some evidence to suggest that early elements of the Yoruba may have used the Benue valley, passing south of Lake Chad, as a migration route on the way to their present habitat.

Ivory work may well have been at some times a notable export commodity at Owo. Plate Fd illustrates a well-known type of kneeling figure, generally attributed to Benin, which I consider to be in the style of Owo. This does not of course by any means exclude the possibility that it and many other carvings in the same style were collected at Benin, for it seems at least possible that Benin at some time recruited some of its best ivory-workers from Owo; this would account for the appearance of certain Yoruba traits in Benin wood and ivory work (but not in bronze-casting), such as the 'Yoruba mouth,' shown as two parallel ridges not meeting at the corners. Plate Fe shows a ceremonial ivory knife or sword in Owo style which was acquired for the British Museum in 1878 by that early connoisseur of African art, Sir Augustus Wollaston Franks, and was stated to be from Lagos. The Owo origin of this piece is established by the presence of a duplicate piece (with several ivory chains attached) in the collection of Mr. Cockin, who informs me that its name is *uda-ira*.

There is a remarkable institution at Owo which may be found to have great significance in African art history: this is a form of the 'second burial' ceremony (widespread among Southern Nigerian tribes) for which as perfect a likeness of the deceased as possible is carved, clothed in the garments of the deceased, and ceremoniously interred some months or even years after his or her death. Mr. Murray and I saw a life-size effigy of the present Olowo's mother which had been carved by the well-known artist Akeredolu and rejected as an insufficiently close likeness in favour of one by Ogunleye. Chief Sheshere has an effigy of his mother by the same Ogunleye, which, though actually used in the ceremony, was not buried but kept in a locked upper room in the chief's house; we compared the head of this carving with a photograph of the deceased lady and found it to be a very close and recognizable likeness. The institution deserves intensive study by an ethnologist, but in the meantime it appears that the association with it of realistic portraiture is an ancient trait and not a recent response to the Photographic Age. The importance of this fact in relation to controversy about whether the Negroes could have conceived the quasi-mensurational naturalism of the Ife bronzes and terra-cottas is obvious.<sup>2</sup>



Now that Nigeria has an Antiquities Service competent to undertake such work, it is much to be hoped that Owo, as well as such obvious sites as Ife and Benin, will before many years have passed be the scene of excavations which may throw light on the relative antiquity of the three traditions and of others with which they may prove to have been connected.

## Notes

<sup>1</sup> A limited number of copies of the illustrated catalogue of the exhibition are still available at the Institute, price 2s. 6d. plus postage.

<sup>2</sup> Indeed, Father Kevin Carroll of Oye-Ekiti has suggested to me that the Ife bronze heads may have been attached to wooden bodies with the same purpose as the Owo figures (the local name for which is *ako*).

## A FLINT BLADE WORKSHOP NEAR GAZIANTEP, SOUTH TURKEY\*

by

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and

W. C. BRICE

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**I25** Some 12 kilometres to the north-north-west of the city of Gaziantep in southern Turkey lies the small village of Duluk, near the site of classical *Doliche*. About two kilometres along the road which leads eastwards from Duluk is a pool and a spring of water called Karpuz Atan; and about 30 yards beyond this, and just to the right of the road, in a vineyard, are to be found numerous blades and prismatic blade cores of fine-grained chert, in various shades of grey, buff and brown. We illustrate here two cores and three blades, selected from the artifacts which we saw there in September, 1947.

In general, the cores are shorter than the blades, since they have been worked to the stage of rejection. All the cores show very similar characteristics, which give a clue

to the technique of blade removal. They generally have simple smooth platforms (figs. 1a, 2a), only a few showing faceting.<sup>1</sup> Usually there is only one platform, but a few have two, at opposite ends of the core. The angle between the platform and flake scar is about 70° (figs. 1c, 2c), and

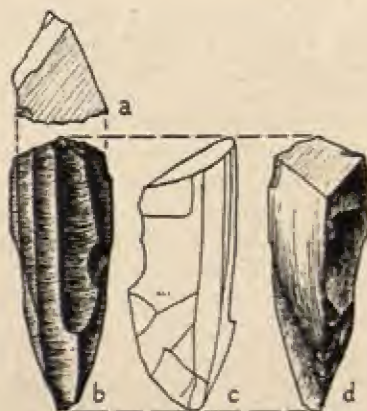


FIG. 1. BLADE CORES FOUND NEAR GAZIANTEP

*Two-thirds of actual size*

\* With four text figures



FIG. 2. BLADE CORES FOUND NEAR GAZIANTEP

*Two-thirds of actual size*

flaking has taken place round only about 30 per cent. of the diameter (figs. 1b, 2b). The reverse of the core (that is, the side opposite the flaking face) is generally prepared by flaking into a sharp ridge or 'keel,' near the base of the core (figs. 1d, 2d). Our experiments at flaking from cores have taught us that one of the chief difficulties of this technique is to hold the core firm, especially as it becomes smaller. So we would suggest that this 'keel' was prepared



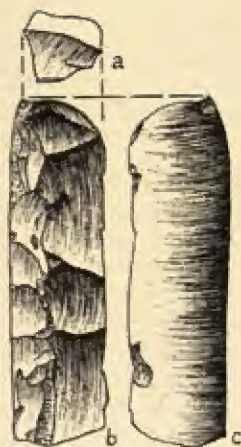


FIG. 3. BLADE FOUND NEAR GAZIANTEP  
*Two-thirds of actual size*

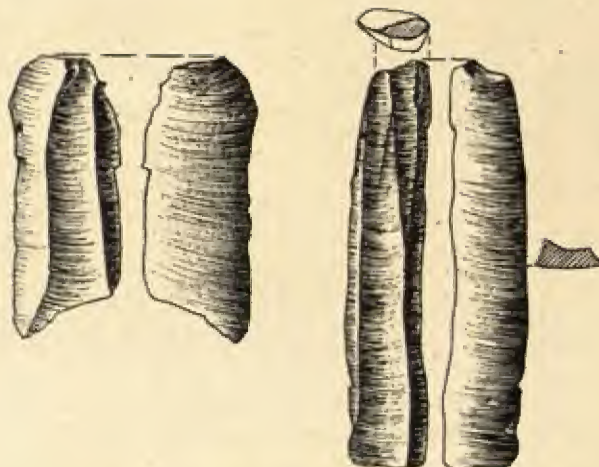


FIG. 4. BLADES FOUND NEAR GAZIANTEP  
*Two-thirds of actual size*

to wedge into a slot or groove in the anvil, to keep the core steady; the frequent signs of contusion on the keel support this conjecture. The keel on the reverse was observed on 32 out of 38 specimens examined.

The special interest of the blades is the evidence they afford of the employment of the technique of the crested guiding flake (fig. 3)<sup>1</sup>. By alternate flaking, a ridge is prepared along the length of the flaking face of the core, and this serves to guide the run of the first blade removed, which therefore has a crested back (fig. 3b) and triangular section (fig. 3a). Blades from the second and subsequent 'layers' are, of course, not crested (fig. 4a, b).

We would like to thank Bayan Sabahat M. Göğüş,

Curator of the Gaziantep Museum. The site is her discovery, and she very kindly took us there, and helped us to collect and record these artifacts.

#### Notes

<sup>1</sup> Facets, to prevent the punch from slipping, may have been prepared on more of the platforms than now show them; but they would be truncated, and eventually lost, as the blades were removed. See A. S. Barnes, 'The Technique of Blade-Production in Mesolithic and Neolithic Times,' *Proc. Prehist. Soc.*, 1947, Paper No. 6.

<sup>2</sup> This technique has been well described by Dr. A. Cheynier and Professor A. S. Barnes in an article, 'Les lames à section triangulaire, et les pièces à crêtes,' in the report of the *Congrès Préhistorique de France*, XII<sup>e</sup>. Session, 1936, p. 630.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**Dual Systems in Australia.** By Dr. Helmut Petri, *Frobenius Institut, Frankfurt-am-Main*. Summary of a communication to the Institute, 9 January, 1951.

126

The dual systems and their importance in the social life of many Australian tribes are well known to anthropologists, and need no further discussion. The question is, whether these social functions of dichotomy are vital to its nature, and whether they could give us valuable information about its historical origins and philosophical background.

From study of a fair number of Australian aboriginal communities in every part of the continent, it can be asserted that exogamous moieties are more than mere mechanisms to control marriage and to establish social groupings; dual systems become significant for the ceremonial and spiritual life of the tribe. Not only the social organization but also the mythical and historical traditions, the sacred songs and corroborees, the burial and initiation rites, and various other items of the blackfellow's culture seem to reflect a 'polar' way of thinking, the primary idea of ordering the whole universe on the principle of antithesis. Essentially antithetic tribal 'halves,' as, e.g., curly-haired and straight-haired, light-blooded and dark-blooded, white cockatoo

and black cockatoo, red kangaroo and grey kangaroo, etc. (in South-East Australia, Western Australia and North-West Australia), without any doubt have a clear tendency to compromise and are of basic importance for the social, economic, religious, historic and ceremonial life of the aborigines. From various tribes we know that the dual system embraces the whole of nature. All the phenomena in the sky and on earth belong either to the one or to the other tribal half (moieties which in their turn may be split up into sections or subsections).

For the Australian native this dualistic constitution always has its origin in the 'dream-time,' in the mythical past, when everything came into existence. Supernatural beings are responsible for this order of things and therefore it may be regarded as an essentially sacred institution of the highest significance for the physical and spiritual welfare of the tribe. The mythical traditions referring to the creation of the moiety system can, roughly speaking, be divided into two versions, in one of which it was a sky hero, in the other a pair of totemic heroes who brought these institutions into existence. It may be of some interest that these pairs of totemic heroes are always conceived as contrary in their thinking and activities. In a way they symbolize the antithetic



human qualities which determine various Australian dual systems, particularly in the Kimberleys and among the Dieri of the Lake Eyre area.

Apparently the social moiety system, combined with the idea of a polar constitution of the world, and the widely spread myth of the two totemic heroes belong together. On the other hand, there are many tribes, particularly in the marginal areas of Australia, whose social order appears to be without any traces of a dual system.

These and other items suggest that a polar way of thinking and a tendency to order social life and nature accordingly need not have had their spiritual roots in the present Australian dual systems, which probably originated somewhere in South-East Asia and reached Australia in comparatively late periods from a historical point of view. The idea of classifying phenomena and the principle of antithesis and compromise may have been common to Australian tribes a long time before new cultural influences came from the north.

#### Some Native Authorities in Nyasaland. By Dr. L. P. Mair.

*Summary of a communication to the Institute, 3 April, 1951*

**127** Kachere and Kaphuka are neighbouring Native Authorities in the Dedza District of Nyasaland, one Ngoni and the other Cewa. It is sometimes suggested that there is a correlation between the efficacy of a Native Authority and the character of the traditional political structure of the tribe. One can relate the greater personal prestige of Kachere with the greater respect of the Ngoni for rank, but his efficiency as a Native Authority is in part due to personal factors such as his experiences in various employments. Kaphuka receives little formal respect, a fact which the Ngoni explain as the result of a system of matrilineal succession, in which a future chief does not grow up in a chief's house; and he carries no weight as a Native Authority. Kachere's council includes some members carrying hereditary rank; Kaphuka's does not. Kaphuka's court clerk has far more drive and intelligence than anyone among Kachere's council or

salaried employees; it may well be therefore that Kaphuka's Native Authority is the more effective local government agency. In fact, as soon as a Native Authority is in a position to employ paid servants, the character of the indigenous chain of command may cease to have much relevance.

The effects of the indigenous method of selection of authorities and of indigenous attitudes towards authority are more relevant for the development of the Native Authority system. The Nyasaland administration would like to make sure that future chiefs go to school. Both Ngoni and Cewa, however, think that the succession should be left open until the death of the holder, and that anyone who sought to prejudge the choice might be murdered by an indignant rival. They also believe that no one should be designated as a future authority until he has proved his character. They still attach value to age as such, and though they will acquiesce in a younger relative acting for a sick or aged chief, they would not favour the succession of the same man to the substantive position.

They believe that authority should be benevolent and paternalistic. Village headmen, who, though they have minor administrative functions, are still essentially a part of the indigenous structure, share this belief and will not transform themselves into policemen, however much some officials might wish it. They conceive themselves as defenders of the village against the outside world, not as its representatives in the village.

In the rural areas, demands for wider representation do not imply demands for political responsibility or democratic control. They are demands for more direct access to the ear of Government. Possibly a more sophisticated attitude is current in Zomba.

Individual Native Authorities have little scope for independent initiative, because (i) they are federated in groups and hence must leave financial control to a central treasurer, and (ii) decisions are sometimes taken, in meetings representing a whole Province, to include items in all N.A. estimates. It is only in the executive field, therefore, that one can speak of relative efficacy.

## REVIEWS

### GENERAL

**Beiträge zur Gesellschafts- und Völkerwissenschaft: Prof. Dr. R. Thurnwald zu seinem achtzigsten Geburtstag gewidmet.** Edited by Ilse Tönnies. Berlin (Mann), 1950. Pp. 478, portrait and text figs. Price DM 25

**128** This substantial volume contains 32 contributions from friends and pupils, representing all the main aspects of the varied career of one of the foremost German anthropologists, which is described in outline by Hilde Thurnwald, and supplemented by a bibliography of 353 published works and eight still in manuscript. Most of the papers are in German, but seven in English (Eberhard, Elkin, Haring, Kroeber, Lowie, Muensterberger, Laura Thompson), further testimony to the width of Thurnwald's travels and influence. Besides numerous special studies of peoples and institutions, there are some of general interest. Of these the most notable is Laura Thompson's study, *Operational Anthropology as an Emergent Discipline* which presents our science as a 'holistic multi-dimensional discipline, coordinator and integrator of all the life sciences.' She lays heavy responsibilities on the 'operational anthropologist' who confronts 'the formative bio-cultural process in total natural setting and historical perspective' or 'the operation of the human creative process in environmental setting.' Well, we can but go on doing our best! The philosophical foundation is the belief, formulated by Smuts, that 'law as formative process is immanent in nature,' superseding the positivist conception of it as mere collection and formulation of recorded instances. It is a creed, as well as a logic; and an applied science as well. As in medicine, from investigation 'standards emerge, whereby the physician may measure the degree of balance or imbalance of a living organism and hence its movement towards or away from health and life,' so in anthropology,

'standards are emerging' for 'the balance or imbalance of a human community in full bio-psychological, cultural and environmental context, and hence its progress towards, or its regression from, an eco-cultural optimum condition.' Truly 'the implications of this approach are revolutionary.'

Sociological studies of lesser scope are those of Marie Baum (the *Welthild* of Ricarda Huch); of Haring on The Social Sciences and Biology; of Lange-Eichbaum, on Ancient Experiences and Thinking; of Karl Müller, on James Burnham's *Managerial Revolution*; of Müller-Freienfels, on the Psychology and Sociology of Writing; of Vierkandt on the Essence and Decadence of Conversation; and of Solms on Personality and Education. An important problem is stated by Wagner: 'Can we predict the Trend of Cultural Change?'

Psychological contributions are those of Kurth on Neurosis as a Psychic Reflection; of Wolf on the Concept of the Archetype and its relations to Ethnology; and of Muensterberger on Artistic Creativity among Primitive Peoples.

Some of the specific studies have also their wider bearing: Hildegard Feick on Cooperative Society in Darmstadt; Knoll-Gröling on the Functions of the Shaman; Rudolf Lehmann on Settlement of Central and South-West African Tribes; Lowie on the Literary Style of the Crow Indians, with some striking examples; and Zachert on Shinto and political leadership in the new Japan.

It is almost a pity that some of the more important articles should be buried in so miscellaneous a collection. There is however a general theme connecting them; and that is the teaching and example of the scholar whom they converge to honour; and whose vivid portrait might be commended by one of these essayists as a 'speaking likeness.'

JOHN L. MYRES



**Psycho-Analysis and Anthropology: Culture, Personality and the Unconscious.** By Geza Róheim. New York (Internat. Universities Press), 1950. Pp. xv, 496. Price \$10

**129** In this massive volume Dr. Róheim tilts at Kardiner, Mead, Benedict, Kluckhohn and in general at the culture-personality school of anthropology. His main contention is that the basic themes described by Freudian psychology, particularly the Oedipus situation, are universal, and that variations of behaviour within different cultures are merely variations on the same theme. To illustrate this he analyses eight different societies, several of them from personal acquaintance, subsequently suggesting that the psychic unity of mankind is based upon the fact that man never fully matures, being what he calls 'a fetalized species.' From this immaturity develop a general fear and dislike of sex, continual search for new objects of emotional attachment, a tendency to psychological regression, a tendency towards ambivalence, a belief in the immortality of fathers and an ingrained conservatism. All these qualities, in Dr. Róheim's opinion, are universal.

The book ends with extracts from and comments on an extraordinary Hungarian philosophical drama called *The Tragedy of Man*, but the matter, at least to this reader, is so turgidly confused that it is hard to say whether it in any way contributes to the argument. The whole manner of writing, indeed, detracts from the book. Aggressive, diffuse, bombastic and polemical, peppered with irrelevant quotations, dramatically italicized, it appears the book of a man who knows himself to be right and is exasperated that the world does not share his certainty.

All the same, given the will to ignore crudities of taste and style, this is a book to read carefully. Beneath the ornate surface can be sensed an encyclopaedic knowledge illuminated by great experience and occasional flashes of insight. After all, the theme of the inner psychic unity of mankind is one which needs reiterating. Much recent work has given us an essential understanding of the relativity of values and of social concepts, so that we may at times feel that the only common denominator of human nature is its plasticity to different cultural moulds. But balance in such ideas may easily be lost, and although a sense of relative values is the basis of tolerance, the idea that people are different has led also to prejudice, and from prejudice to persecution.

It only remains to question whether Dr. Róheim illustrates underlying psychological unities in the most convincing fashion.

ADAM CURLE

**Sociologie et Anthropologie.** By Marcel Mauss, with an introduction to his work by Claude Lévi-Strauss. Paris. (Preses Universitaires de France), 1950. Pp. lii, 390. Price 800 francs

**130** This is by no means a full collection of the voluminous writings of Marcel Mauss, who died while this book was in the press; but the essays which it contains are selected as illustrating the principal aspects of his thought. How they are connected, M. Lévi-Strauss expounds in an essay so elaborate that it is better read after the writings of Mauss himself: it gives however some essential dates and cross-references to the work of Durkheim and other collaborators. The 'Sketch of a General Theory of Magic' (1902-3; pp. 1-138) proposes a definition, analyses the elements, and explains the functions of magic, with comparisons very widely sought and ingeniously correlated. It was written in collaboration with H. Hubert, one of Mauss's chief associates in the *Année Sociologique*.

The 'Essay on Giving: Forms and Reasons for Exchange in, Archaic Societies' (1923-24; pp. 143-250) examines first the gifts exchanged, and the obligation to exchange them; the extensions of this obligation, in liberality, sense of honour, and the emergence of money as measure of values; and the survivals of these principles in ancient laws and economics.

'The Relations, Real and Practical, between Psychology and Sociology' (1924; pp. 281-310) treats of the place of sociology in anthropology, the recent services of psychology to sociology, and conversely, and proposes some questions to psychologists.

'The Physical Effect of the Idea of Death on the Individual' (1926; pp. 311-30) deals mainly with the collective societies of Australia, New Zealand and Polynesia, and illustrates the preceding relations between psychology and sociology.

'The Notion of "Person," of "Myself,"' (pp. 333-64) is the Huxley Lecture of 1938 (*J. Roy. Anthropol. Inst.*, Vol. LXVIII). It begins with the ritual mask (*persona*) which identifies the wearer with his performance, and rises to the metaphysical and moral conception of a being with conscience and supernatural value. What further development of meaning lies beyond? The question is serious, for from various sides the notion is challenged, and therefore with its moral and social functions.

'The Techniques of the Body' (1934; pp. 363-86) is lighter stuff, but illustrates a rare type of anthropological study. Every group has its own way of using its limbs: French and British infantry march quite differently. Such habits may be carefully taught, and socially admired. They cover all phases of life and all kinds of activity. Such teaching is a crucial instance of the relations between a society and its members, of which Mauss gives amusing instances; and I remember him illustrating by such an instance the international stresses of the modern world: which is the *right* way of using knife and fork? And, as he ends here (p. 386) on the postures of prayer: 'je pense qu'il y a nécessairement des moyens biologiques d'entrer en communication avec le Dieu.' JOHN L. MYRES

**Mythe, Mensch und Umwelt: Beiträge zur Religion, Mythologie und Kulturgeschichte.** Edited by Ad. E. Jensen. *Paideuma*, Vol. IV, 1950

**131** This number of the journal of the German Society for Culture Morphology is also the jubilee volume of the Frobenius Institute of the Goethe University of Frankfurt-a-M. As such it naturally includes a summary history of the Institute and its many expeditions to Inner Africa, as well as a Frobenius bibliography. This is not the place or the occasion to review controversies that gathered around Frobenius and his adventures and we may simply say that all researchers will be glad to know that the Institute is able once more to become active in scientific work. The volume is a typical *Festschrift* with many unrelated contributions and a reviewer can but note some of them.

Heine-Geldern attributes the black and the grey ware of the Lung-shan culture of pre-Shang China, as well as the several varieties of painted pottery of North China, to steppe-land and ultimately perhaps South-West Asiatic influences, including actual migration of peoples. One may suggest that in South-West Asia we are still groping for clues that may be revealed when there has been more excavation of the great tells in the region in which the Euphrates enters the lowland.

Wölfel emphasizes the relationship of the archaeological data of the Canary Islands to those of Mediterranean Islands and Berber Africa and speculates on relations with the Guinea Coast.

Weisweiler contrasts the Ulster and Leinster (Finn) sagas. The former is full of Mediterranean parallels emphasizing cattle, the latter of Northern parallels emphasizing deer and the hunt. It might be pointed out that the megaliths of the north-east of Ireland are largely 'gallery' graves with probable Mediterranean affinities, whereas the dolmens of southern Ireland have yielded beads of Danish amber but never yet a bead of callais, which is so important in Morbihan, Portugal and Mediterranean France.

H. J. FLEURE

**Das religiöse Weltbild einer frühen Kultur. Studien zur Kulturkunde, Vol. IX.** By Ad. E. Jensen. Stuttgart, 1948. Pp. xii, 190 with 4 illustrations

**132** This close-knit essay consists of three parts. Issued under the auspices of the late Leo Frobenius, it is designed to illustrate and support his ethnological theories and methods; it sets out comparisons between widely distributed cultures, in regard to some of their most deep-seated notions, and claims a 'historical' connexion between them, and it constructs, on the basis of these comparisons and derivations, a coherent system of primitive ideas, the 'lunar culture,' of which these related cultures are claimed as inherited survivals.

The first part is of antiquarian and mainly sentimental interest, for it does not really matter much now whether Frobenius or Gräbner and Ankermann invented the doctrine of *Kulturkreise*. Those who are old enough to have been influenced by the tortuous



writings of Adolf Bastian know very well whence *they* derived these notions. Nor does it matter much how far Frobenius himself changed his mind, even if that improved his theory. Frobenius had an unusually vivid imagination, and saw resemblances easily and on very slight occasions; and herein his followers resemble him. Hence perhaps their meticulous efforts to prove their comparisons valid.

For the general reader, reference may be made to the section in this book on the Eleusinian Mysteries, based on the essay of Walter F. Otto, to whom Jensen dedicates it. It was Felix Speiser who first suggested that Demeter was worshipped with offerings of pigs because in early times people had no other offerings to make. Otto and Kerenyi have elaborated this and similar observations, and introduced Medusa, Isis, and other figures to Eleusis. The comparisons between Eleusis and ceremonies at West Ceram, Marindanim, and Kiwai in New Guinea; the Indian Khonds, the South-Rhodesian rock-painters, the Pangwe studied by Tetsmann, and several Mexican and South American peoples are ingenious but not always convincing. There is some suggestive discussion of the parts played by dramatic representations as well as oral tradition in such cults.

How are such far-flung resemblances to be explained? Jensen discusses destructively the alternative psychological theory, represented by C. G. Jung, that similar notions recur in dreams or speculations among separate peoples. Quite apart from the theory popularized by Lévy-Bruhl, that primitive folk had a different mental procedure from ours, such sources for these similar notions lack the central idea which in the 'historical' theory alone makes them relevant and intelligible. It is the complex, the *Kulturkreis*, that gives significance to the ideas which compose it. This contention ingeniously disposes of the numerous instances where one or more of such a group of ideas appear, but not others; but at first sight it devastates the *Kulturkreis*, if its components occur outside as well as within.

Birth and death, and especially procreation, however, seem to be everywhere of exceptional significance, and therewith the act of killing, the converse of procreating. On these facts rests the mythical view of the whole world, which these observances and beliefs express. It is not at first sight clear whether the recurrence of this system of ideas supports the 'historical' or the 'psychological' hypothesis; even if, as Jensen proposes, attention is confined to this one group of related ideas—what he calls the 'lunar culture' from the prominent, if not central, position of lunar cults and observances within it. For the more universal and fundamental these lunar elements are, the less does it seem necessary to connect them with pig-keeping, head-hunting and the other landmarks of the lunar culture.

At the end of the book is a convenient English summary; but it does not clear up questions not answered in the essay itself.

JOHN L. MYRES

**Fishing in Many Waters.** By James Hornell. C.U.P., 1950. Pp. xv, 204, 26 plates and 44 diagrams. Price £1 10s.

**133** The late Mr. James Hornell, who was the acknowledged authority on the canoes of Oceania and the author of many books and pamphlets on nautical research and kindred subjects, wrote, in *Fishing in Many Waters*, what is probably the first book of its kind on the little-known fishing methods in many parts of the world, notably in India and the South Seas. Here is a really memorable book. There is much in it to interest the student of nautical research, and the anthropologist will find it a valuable contribution to material culture. Mr. Hornell had a very marked descriptive ability and an untivalled personal knowledge of the areas covered by his book. Most of his material was gathered in the course of fishing missions on behalf of the Colonial Office, and the majority of the very beautiful photographs that illustrate the book are his own. In format and general lay-out it is a companion volume to his previous work, *Water Transport*, and it is up to the same high standard.

Chapter 1 deals with weapons of the chase borrowed by the fisherman, such as the spear, harpoon and crossbow; this is followed by animals trained to fish and fishes that angle for their living. Most people have heard how cormorants are used for fishing, with the

bird's neck constricted by a ring to prevent its swallowing the fish which it has caught. It will be a surprise to some people to learn that trained otters are used for the same purpose. Shark-fishing is dealt with briefly in Chapter 4 and this is followed by sea fishing in West Africa and the little-known kite fishing. The pursuit of the bonito, the albacore and the tunny is the subject of Chapter 7. Next to be dealt with is a description of baiting for crocodiles and alligators.

Lovers of curry will be interested to learn of the methods and superstitions of the fishermen who trawl for the famous *bunla*, or 'Bombay Duck.' Chapter 10 deals with the habit common to several species of grey mullets of leaping from the water to surmount an obstacle in their path and the various ways of catching them in the process. The brief description of the so-called 'mirror-board' device does less than justice to the Chinese fisherman; actually this method of fishing as practised at Ningpo and at the village of Wang Chau is much more ingenious than Mr. Hornell's description suggests.

Netting for the Indian shad finds incidental mention in Chapter 11, while Chapter 12 is devoted to fishing at Port Said. This is followed by the methods of catching flying fish off the Indian coast and various methods of trapping fishes. It is certainly news that the octopus is intelligent, but Mr. Hornell makes a good case for this view in Chapter 15. The chapters on eel farming and poison fishing are most valuable contributions. Naturally Mr. Hornell has much to say about pearl fishing, for he was for several years in charge of the pearl fisheries carried on by the Government of Ceylon. The book ends with a description of 'Fruits of the Sea.'

This book is, of course, primarily a textbook and as such gives the fullest details of the methods used. If there is any criticism to be made it is that in spite of Mr. Hornell's knowledge of Chinese shipping it is sad that he has omitted all reference to the bamboo casting reel which the Chinese claim to have invented and to pisciculture which they certainly were one of the first nations to understand. It is disappointing, too, that there is no reference to the huge fishing industries of the Chusans, Ningpo and Hong Kong.

G. R. G. WORCESTER

**The King and the Corpse: Tales of the Soul's Conquest of Evil.** By Heinrich Zimmer (ed. Joseph Campbell). Bollingen Series XI. Washington (Pantheon Books) (U.K. agents: Allen & Unwin), 1948. Pp. ix, 316. Price 18s.

**134** This posthumous volume is a collection of folk tales and myths from Mediæval Europe, the Near East and India. The author traces some extremely interesting links between pagan mythological motifs and Christian legends, and offers an ingenious interpretation of many features of the epic cycle of King Arthur and his Round Table. He contrasts the ideals of the archaic Celtic tradition with those of the classical Greek and the Christian ideologies, and points out that the Celtic attitude towards the demonic-superhuman distinctly suggests the archaic Orient rather than the later Occident. Phœnician tin trade with Cornwall is considered one of the channels through which 'the symbols and tales of the pre-classic Egypto-Babylonian civilizations were . . . carried directly to the pre-Celtic and Celtic populations of Britain, Wales and Ireland.' Anyone interested in comparative mythology will find this collection of legends and their interpretation very stimulating, and it is to be regretted that in the chapters on Indian myths the comments and interpretations are slighter than in the earlier part of the book, a deficiency undoubtedly caused by the author's untimely death.

C. von FÜRER-HAEMENDORF

**Die Textilsammlung Fritz Iklé-Huber im Museum für Völkerkunde und Schweizerischen Museum für Volkskunde, Basel.** By Kristin and Alfred Bühler-Oppenheim. Denkschriften der Schweiz. Naturforsch. Ges., Vol. LXXVIII, Part 2. Basel, 1948. Pp. vi, 83-267, 3 plates, 159 illus.

**135** Museum curators seldom give an enthusiastic welcome to large collections of basketry and textiles, for, as a rule, they are cumbersome and moth-attracting, difficult to display and of interest only to experts. But the Iklé-Huber collection is an exception, and the Basel Museum must be congratulated on possessing such a splendid mass of material, and still more on the production of what the



authors modestly describe as a catalogue, but what actually constitutes a technological encyclopædia of the textile arts.

The book is divided into three sections: the techniques of weft-making (*Fadenbildung*), of material-making (*Stoffbildung*), and of decoration and make-up (*Stoffverzierung und Stoffverarbeitung*). The first section starts with the raw materials, stems, roots, leaves, hair, sinews, etc., and the processes whereby these are made into wefts for combining into *Stoff*. *Stoff* includes the whole range of plaited or woven objects from a wickerwork basket to an Oriental carpet. The technique is clearly illustrated by means of diagrams (some of incredible complexity) and photographs. The technical terms are those already familiarized by one of the authors in her monograph on the textiles of New Caledonia and the Loyalty Islands in 1942 (*Int. Arch. f. Ethnog.*, Vol. XLI, Suppl.). In this section we have the various processes of knitting, netting, knotting, hooking, plaiting, twining, coiling, etc., leading on to weaving, card-weaving, simple weaving frames and looms. The third section is mainly concerned with ornamentation, edgings, fringes, and other trimmings, also with stitchings and embroideries, the addition of paint, wax, etc., tied work, *batik* and *ikat*.

It is only with the help of a world-wide collection such as that at

Basel that a world-wide survey can be made, and it is surprising to find complicated techniques at opposite ends of the earth. 'Soumak weave,' so called from its discovery in the Caucasus region, reappears in ancient Peru. A form of twining links Austria with New Zealand and Paraguay. Under *Kettenflechten* we have examples from Bosnia, Peru and Indonesia, together with illustrations of the apparatus for making the arm bands of Santa Cruz and the Solomon Islands whose unravelling has often puzzled the experts.

Special emphasis is laid on the valuable examples from ancient Peruvian sources, in which the collection is extraordinarily rich. The authors confine themselves strictly to technique and to the more important types of technique. It is good to learn that the artistic styles, characteristics and analyses of patterns are being studied in preparation for a further volume. The beauty of ancient Peruvian work has long been recognized, and the analysis of its technical intricacy increases our admiration. Though only the expert will appreciate the diagrams, those who look at the pictures will find much to enjoy. There are three coloured plates of ancient Peruvian and Bolivian textiles; the detailed list of contents serves as an index, and there is a bibliography of 257 items.

A. H. QUIGGIN

## PHYSICAL ANTHROPOLOGY

**This is Race.** Edited by Earl W. Count. New York (Schuman), 1950. Pp. xxix, 747. Price \$7.50

**I36** The word 'race' usually implies a group of people whose members resemble each other more closely than they do those of other groups. Attempts to define races scientifically have been as diverse as they have been numerous, and controversy still continues over a variety of problems, including the methods to be used in differentiation, the fineness of the sub-divisions, and the biological meaning which is to be attached to the ultimate products of the analysis. Renewed attention has recently been focused on these topics by U.N.E.S.C.O.'s memorandum on 'Race'.

There is a considerable literature of the subject, and *This is Race* attempts to place the 'scientific concept of the races of man' in historical perspective by presenting in chronological order a 'light sampling' of the relevant literature which has accumulated between 1749 and 1944. However, even this 'light sampling' consists of 60 contributions which extend to 700 closely printed pages. The editor of the volume, Dr. Count, is both a cultural and a physical anthropologist, and is head of the anthropology department of Hamilton College, Clinton, New York.

The book contains articles by authors of 12 nationalities. Some are published in English for the first time. The book deliberately excludes general human phylogeny, because the relevant literature is too voluminous. Comparative racial physiology and psychology are also excluded, because knowledge of them is 'still too far from even fair crystallization to warrant inclusion in so brief an anthology.'

The first section covers the period from the mid-eighteenth century to Darwin's time, and includes works by Buffon, Kant, Blumenbach, Lamarck, Cuvier, Broca, Retzius and others. These outline the early attempts to classify man on the basis of a few morphological, and in some cases cultural, characters, and summarize classical discussion of the question whether living man represents one or several species. Views on the phylogenetic significance of these sub-divisions were, at this stage, generally obscured by the current theological doctrine of man's special creation.

The centre section of the book refers to the period after the publication of Darwin's theories, and is permeated by the general idea that 'races' in man are just as much natural evolutionary units as they are in other animals (e.g. Darwin, Huxley). This idea leads to the discussion of man's monophyletic or polyphyletic origin (e.g. Fritsch), and to a consideration of the basis of raciality—whether they emerge as a result of the action of natural selection on heritable variability, or whether they are due entirely to the effects of environment (e.g. De Bréau, Virchow, Haddon, Fischer). Further argument focuses on the use in classification of single characters or groups of attributes (e.g. Virchow, Ripley, Sergi). Interspersed among these discussions of general theoretical topics

are a variety of views about the number and extent of the human races.

The last part of Dr. Count's anthology covers the period extending from the beginning of the First World War up to 1944. It is marked first and foremost by a considerable extension and refinement of the quantitative methods which were first used by Retzius (e.g. Morant). In this period there was also a greater tendency to use combinations of characters to classify man, and, in common with what was now the general practice in zoological taxonomy, studies were made of man's geographical distribution. Some anthropologists (e.g. von Eickstedt) even advanced the general idea that man forms a polytypic species. Still more recently, attempts have been made (e.g. by Boyd) to classify on the basis of small groups of presumably non-adaptive characters whose mode of inheritance is both simple and precisely known—in this case the blood groups.

The result of this diversity of anthropological method is still a bewildering variety of opinions as to the number and extent of the groups of man worthy of the status of 'race.'

During the past twenty years the union of several zoological disciplines has resulted in comprehensive theories about the underlying basis of evolutionary change, and has provided methods for determining taxonomic groups in such a way that their evolutionary significance may be assessed. Although it only covers one aspect of human evolution, Dr. Count's book not only indicates the need for a corresponding synthesis of the disciplines which together constitute the subject of 'human biology,' but also shows that many of even the fundamental concepts of the 'new zoology' have still to penetrate the outer defences of the fortress of physical anthropology.

E. H. ASHTON

**Human Ancestry from a Genetical Point of View.** By R. Ruggles Gates. Cambridge, Mass. (Harvard Univ. Press) (London: Cumberlege), 1948. Pp. xvi, 422, 27 plates. Price £2 2s.

**I37** Professor Gates has attempted the difficult task of surveying, in 400 pages, the wide fields of human palaeontology and the origin of the modern races of man. The main purpose of the book is to show that the modern races of man belong to at least five distinct species, in the biological sense of this word. These species have arisen at varying times, from different ancestral species in different parts of the world, and have developed their common features through the processes of parallel evolution and repeated parallel mutations. The first chapters of the book show the importance of these processes in evolution, from primitive life to the primates. The value of intersterility as a criterion of differentiation into species is discussed and it is shown convincingly that intersterility may occur early or late in the evolution of species from a common ancestor and that, by itself, it is no unique criterion of difference of species.



The early part of the book deals with the palaeontological evidence of man's ancestry. The important skeletal remains are described, more or less in the manner of the physical anthropologists, and are shown to belong to different species and different genera. There is an important chapter on head shapes and their inheritance. Many of the characteristics of the skull can be considered as genetic units and undergo independent variation. The mixed features of some skulls may thus be due to interbreeding of different species of primitive man. Professor Gates considers the evidence to suggest two or three independent streams in human evolution with occasional crosses between them. The two main lines, on the basis of the

skulls, he calls goriloid and orangoid, while the third possible line is the Australopithecinae. In later chapters, the author considers in more detail the path of human evolution in the main parts of the world and gives his own scheme of human phylogeny. The emphasis always is on the elucidation of race relationships by the application of genetic principles and much use is made of blood-group data.

This is not an easy book for the general reader, as much of the argument presupposes a good knowledge of physical anthropology and genetics. For the specialist, however, the book should prove interesting and stimulating. M. LUBRAN

### ARCHAEOLOGY

**Man the Tool-Maker.** By Kenneth P. Oakley. British Museum (Nat. Hist.), 1949. Pp. 98, 2 plates, 41 figs. Price 2s. 6d.

**138** The British Museum handbooks have always been regarded as expositions of orthodox thinking and since they are written for lay readers the style and form has to be clear and simple without losing sight of fundamentals. Dr. Oakley follows competently in the footsteps of his predecessors in compiling a guide to the early human artifacts at the British Museum of Natural History.

In the words of the preface, Dr. Oakley gives 'a palaeontologist's view of the culture of early man' and certainly his writing has a savour of the natural historian's approach. On this account his conception of orthodox opinion will no doubt be criticized by those prehistorians who aver that prehistory is a social science, whatever that may mean. But it is this very discipline in natural history which gives Dr. Oakley's exposition a clarity and conciseness often absent from the writings of some of those who claim a better insight into prehistory because of their so-called social approach.

Dr. Oakley summarizes his views on the evolution of culture (he uses the term loosely) in a table attached as an appendix. He sees a first major division of industries into those which have hand-axes and those which have not and then shows, by way of a three-dimensional representation, how these stems developed, migrated and blended in each of the three continents of the Old World. The handaxe stem evolved in Africa, from the Oldowan-Kafuan to the Acheulian and thence to the Tumbian. In Asia the flake and chopper-tool stem developed through the Soan-Choukoutien to the late Soan and its derivatives. The Acheulian came into Asia by way of South India, and also it entered Europe, where it developed, in contact and blending, with a Clactonian derivative of the flake and chopper-tool stem. The early Mousterian, arising in Asia as a result of the blending of the Soan and Acheulian, spread outwards into Europe and Africa in the third inter-glacial stage. (Dr. Oakley uses a simple numerical chronology referring to first, second and third inter-glacial stages and recognizes that the fourth glacial stage is divisible into two, thus following Geikie in his original terminology of the glacial sequence.)

He goes on to describe how the Aurignacian developed from these blended stems in Asia and spread westwards into Europe during the last glacial phase, there giving rise to the Chatelperronian and Gravettian, with intervening Solutrian and final Magdalenian. Contemporaneously the Caspian, Aterian and Middle Stone Age complex developed in Africa.

Superimposed upon this three-dimensional representation are lettered symbols for the associated remains of fossil man which are grouped into what Dr. Oakley calls stocks. One stock, *Homo sapiens* proper, developed in Africa and is associated with the Acheulian handaxe tradition; the *Pithecanthropus* stock developed in eastern Asia and is associated with the Soan complex. Both stocks sent offshoots into Europe and are there represented by the Piltdown skull (derived) and Heidelberg man. The Neanderthal stock evolved in Asia, perhaps out of the *Pithecanthropus* stock, and migrated into Europe and Africa, where it blended with the indigenous *Homo sapiens* and derived *Pithecanthropus*. *Homo sapiens* gained ascendancy in south-west Asia and from there spread out to dominate the Old World. Dr. Oakley makes no reference to the source from which he derives this system of representation and the use of the words 'stem' and 'stock.'

There is a simple and clear description of the techniques and

materials in stone-working, and the author hazards some conclusions about the evolution and utilization of stone tools. For example he suggests that handaxes were not used for digging but for cutting and scraping. It is presumably his orthodoxy which prevents him from discarding the term handaxe. He believes that the important changes are to be seen in the development of cutting tools but neglects the possibility that major migrations might have demanded the development of weapons such as those used by the Mousterian people, giving them their ascendancy in the third inter-glacial stage.

In describing the art forms of the Upper Palaeolithic Dr. Oakley's discipline in natural history is inadequate. He notes that the mural art is almost entirely concerned with food and that many of the paintings have little regard to previous work; but from this objective statement he is led into speculation, and assumes faith in the idea that this art is related in some way to the practice of sympathetic magic. This is not his fault, for he is but following those who would decry his discipline of thinking. There is a tendency to forget that if our interpretation of prehistoric industries is dependent upon analogy and comparison with industries of the present, it follows that the prehistorian must use the same method in considering mural art. Societies exist today where the art shows a predilection for themes consistent with the general ethos, where the best products are preserved on the walls of caves or artificial caves (museums), where original paintings are frequently obscured by later work, and where rejected or second-rate daubs are confined to the domestic site. In other words we see aesthetic appreciation of such a kind that artists are encouraged and their best products preserved. Such appreciation was not unknown at intervals in historic and prehistoric time and it would be conceit on our part to suggest that the men who performed these wonders on the walls of caves in Spain and southern France had less aesthetic appreciation than we ourselves. In other words cave art demonstrates the existence of aesthetic appreciation in Upper Palaeolithic times and it is not necessary to go beyond this and indulge in speculation about sympathetic magic unless we regard our own art forms as being inspired in a similar fashion. If we do so we enter a field of deep psychology where prehistorians are incompetent.

It is likely that Dr. Oakley's book will be as useful and come to be regarded with the same affection as the late Mr. Reginald Smith's, which was also enlightened by Mr. Waterhouse's excellent drawings.<sup>1</sup> T. T. PATERSON

Note

<sup>1</sup> A revised edition of this work has been published since Dr. Paterson's review was completed.—Ed.

**A Study of Archaeology.** By W. W. Taylor. *Memoir 69 of the Amer. Anthropol. Assn. Menasha, Wisconsin, 1948. Pp. 256. Price £1 6s.*

**139** There have hitherto been very few studies of the methods of archaeology or of the conceptual basis of prehistory. This book is welcome for that reason alone, but it is also welcome as a most distinguished, thoughtful and thought-provoking study. Professor Taylor pictures archaeology as confused in its theoretical structure, its aims and its relationships to other subjects—claiming to be anthropology, history, the study of art. He attempts, if not to clear this confusion, to make us painfully aware of it. Part I of the book is an analysis of the history and present status of American archaeology. The main burden of the argument is that 'the archaeologist of



today is a Jekyll and Hyde, claiming to "do" history but "be" an anthropologist without attempting to determine the relationships if any between these two fields of study as they pertain to his own investigations.' He stigmatizes modern archaeologists in the main as excavators, and writers of description with no sense of problem. Part II consists of a discussion of a sound conceptual basis for archaeology and the presentation of a series of practical procedures for archaeological research.

The perspective which Taylor advocates is what he calls the conjunctive approach, as distinct from the approach currently practised in America, which he terms the comparative or taxonomic approach. By the conjunctive approach he means the description of the cultures of past human groups: it has, he says, as its primary goal 'the elucidation of cultural conjunctives, the associations and relationships, the "affinities" within the manifestation under investigation' and aims at 'drawing the completest possible picture of past human life in terms of its human and geographic environment.' Taylor's analysis of the concept of culture in archaeology is most valuable.

Taylor's book is mainly directed at American archaeologists but many of his criticisms apply equally well to European archaeologists, some of whom are concerned entirely in 'the building of chronological sequences and so-called culture classifications with purely taxonomic inferences.' On the other hand, European archaeology does provide, in the persons of such scholars as Gordon Childe and Grahame Clark, to mention only British examples, admirable exponents of Taylor's conjunctive approach. And the conjunctive approach, admirable as it sounds, cannot be practised unless excavation and fieldwork have produced the facts to be integrated. A very important book, which should be read carefully, and pondered over by every European prehistorian—and archaeologist.

GLYN E. DANIEL

**Some Applications of Statistics to Archaeology.** By Oliver H. Myers. Cairo (*Service des Antiquités de l'Égypte*), 1950. Pp. 37, 19 figs. Price P.T. 120 (£1 5s.)

**I40** It is somewhat surprising that in archaeology, a discipline in which variability and association play a fundamental part,

**La Vie sexuelle en Afrique noire.** By D. P. de Pedrals. Paris. (*Payot*), 1950. Pp. 188. Price 420 francs

**I41** M. de Pedrals has written a sincere and sympathetic survey of sexual life in Negro Africa in its physiological, cultural and social aspects. He catalogues many of the relevant customs, though there is little discrimination in his selection and evaluation of source material. He has a basically Frazerian outlook, and his main argument is that African concern with sex may be a reflex of a more general concern with fertility in all its aspects.

LAURA BOHANNAN

**Préhistoire et Protohistoire d'Égypte.** By E. Massouard. *Trav. et Mém. de l'Inst. d'Éthnologie*, Vol. LIII, Paris, 1949. Pp. xxviii, 567, 3 maps, 110 plates, text figs.

**I42** Dr. Massouard has given us an excellent survey of Egypt from Palaeolithic times until the end of the Second Dynasty. This is written simply and clearly, and contains extremely extensive descriptions of every kind of evidence, arranged in a manner which greatly facilitates the study of the periods with which it is concerned. The writer shows himself to be remarkably objective in his attitude, most refreshingly so in fact, and as a result readers will probably feel confident that he will not lead them far away from any rational position in any direction by insinuating preconceived notions. He is, however, inclined to treat Egypt as an entity in itself, rather than as fundamentally part of the Near East, while enjoying a highly developed individual culture. His view is, of course, the traditional one, and should, perhaps, not be strongly opposed, though equally

few attempts have hitherto been made to apply the techniques of statistical analysis and correlation. Mr. Myers' monograph is thus to be welcomed as an effort to illustrate the possible value of this neglected approach.

Four problems are here examined, based on material from excavated and surface sites in Nubia, Aden and Egypt: first, the quantitative relationship of stone tools and pottery on a long-occupied site; second, the value of the average size of sherds from desert sites, as a criterion of the quantity of pottery originally in use; third, the variation in hardness of ancient pottery, particularly as it is affected by exposure in the desert; and lastly, the correlation of types of artifact on surface sites, occupied successively by different communities, in which stratification is absent.

The last of these problems receives the fullest treatment, and yields interesting results. The author shows that if certain initial assumptions are made concerning the size and random distribution of the successive units of settlement (huts), unstratified material can be recorded and analysed in such a way that 'each series of flints could be allotted correctly to its associated pottery,' with an order of accuracy comparable with that obtainable on stratified sites. This technique is clearly best fitted to desert sites, but might usefully be applied to analogous material in Europe, within the limits of validity of the initial assumptions.

The author expresses the hope that his work will encourage a similar approach among archaeologists elsewhere. It is the more regrettable, therefore, that in presenting this illustration of the method, no concessions have been made to the ordinary reader's unfamiliarity with statistical techniques. The use of the terminology and mathematical symbolism of statistics, without adequate explanation, is likely to deter rather than to encourage the reader. Some of the paper wasted in the excessively wide margins of this monograph might well have been used to explain to the non-specialist the principles underlying the method.

Finally, it must be noted with regret that Mr. Myers has been ill served by his publisher and proof-readers. Errors in spelling, punctuation and page references occur far more frequently than is proper in a government publication. R. J. C. ATKINSON

## AFRICA

the objections to it should, it may be, not be evaded to quite the extent that occurs in this book. His work is undoubtedly deserving of very high praise, and his ability to take a wide view, his seriousness and his avoidance of strong prejudice will be of the greatest value to all, and especially cannot fail to influence young students for their good.

The illustrations are drawings, perhaps a trifle roughly executed, but quite adequate to serve as *aide-mémoires* until the reader can look up photographs of the objects referred to in the book.

T. BURTON-BROWN

**Témoignages des Sources classiques sur les Pistes menant à l'Oasis d'Ammon.** By Jean Leclant. *Bull. de l'Inst. français d'Archéol. orientale*, Vol. XLIX (1950), pp. 193-253, with 1 map

**I43** The Ancients wrote little on this subject, although the famous temple of Ammon in what is now the oasis of Siwa, was a favourite place of pilgrimage for a thousand years from the sixth century B.C. onwards (Alexander being its most famous consultant).

The sources are grouped under several heads: the Desert, country of sand and thirst; the danger of the South Wind; the providential or terrific animals; scientific observations on the surrounding country; and data on itineraries, horaries, conditions of travel.

M. Leclant himself points out how deceptive these short references are in the attempt to reconstruct the organization of caravans and the trade of the oasis. But his work will greatly help those who are interested in traffic and commercial routes between Egypt and the Western Desert in classical times. RAYMOND MAUNY

## ASIA

**The Problem of Similarity in Ancient Near Eastern Religions.** *The Frazer Lecture, 1950.* By H. Frankfort. Oxford (Clarendon Press), 1951. Pp. 23. Price 2s.

**I44** The Faculty of Anthropology of the University of London has honoured itself in selecting Professor Frankfort to

deliver the Frazer Lecture for 1950. Before his welcome accession to the ranks of British scholars as Director of the Warburg Institute, his distinction as a field archaeologist was generally recognized, and his *Cylinder Seals*, published in 1939, when he was Research Professor of Oriental Archaeology at Chicago, is a most valuable piece



of work of abiding importance. The publication of the symposium entitled *The Intellectual Adventure of Ancient Man*, republished as a Penguin book, and his recent book *Kingship and the Gods*, revealed him as an anthropologist who was concerned to give us a much-needed demonstration of the valuable contribution which archaeology could make to the younger discipline of anthropology. Now it is as an anthropologist that he has, in the Frazer Lecture for 1950, delivered a resounding broadside against what he regards as certain deadly heresies. Indeed, the title of a famous treatise of Irenaeus, *Adversus Haereses*, might appropriately serve as the title of this lecture.

The two heresies regarded as fundamental by the lecturer and subjected to his most devastating attack are, first, the view, based on Frazer, that differences are specific and similarities generic, and, secondly, the claim that one definite pattern underlies most of the religions of the ancient Near East. The *corpus vile* in which these heresies are alleged to be embodied in their most dangerous form consists of various books published between 1927 and 1936 and named by Professor Frankfort as outstanding examples of the tendency which he is concerned to expose and refute. Of the authors thus pilloried some are dead, while others still survive and are no doubt capable of defending themselves. But it may not be irrelevant to remark that the modern habitat of the deadly 'patternist' hydra is no longer in this country but in the Lernaean marshes of Scandinavia. There the patternists are strongly entrenched and might perhaps have provided a worthier object of Professor Frankfort's prowess. But no doubt their turn will come.

It is obviously impossible to deal satisfactorily with the patternist controversy within the brief limits of a review, and our attention must be confined to a few major points on which some may feel that the lecturer's somewhat pontifical utterances are open to question. First, from the point of view of logic, as well as from that of scientific method, it can hardly be doubted that similarities are generic and differences specific, and the fact that similarities may be over-emphasized or improperly defined does not invalidate the fundamental rule. At the time when the books which Professor Frankfort attacks were written, much less was known about the field of Near Eastern archaeology than now; the study of the Rās Shamra material was in its infancy; Professor Kramer's researches into Sumerian mythology had not yet been published, and many other new sources of knowledge were not yet available. A quarter of a century is a long period of time in view of the rapid rate of modern scientific progress. Today, thanks largely to the archaeological activities of scholars such as Professor Frankfort, a vast amount of fresh material is available for study, and by a natural swing of the pendulum our attention is being focused on the important differences which exist between the various contemporary and related cultures of the ancient Near East. We are greatly indebted to Professor Frankfort for his brilliant demonstration of the essential differences between the culture patterns of Egypt and Babylonia, but this does not of necessity involve the denial of a wider underlying generic similarity embracing all these ancient cultures which rest ultimately upon an agricultural basis, a similarity whose discovery was the aim of those earlier tentative studies so strongly condemned by Professor Frankfort.

This brings us to the second of the heresies attacked by the lecturer. With regard to this two things may be said. In the first place, so far from being a slavish following of Frazer, the movement represented by the various studies already mentioned was in reality a revolt from the Tylorian and Frazerian assumption that similarities in separate cultures were the result of the essential similarities in the working of the human mind, wherever found. Indeed, the original impetus to the studies in question was an aftermath of the now extinct diffusionist controversy. One of their main objects at that time was to put forward the view that the observed similarities between the various culture areas of the ancient Near East were largely the result of culture contacts. Incidentally it may be remarked that the first paragraph of *Myth and Ritual* was a criticism of Frazer's use of the term 'primitive.' In the second place, whatever subsequent exaggerations of the original theory may have been produced by later 'patternists,' the first suggestion of a 'myth and ritual pattern' was never intended to be taken in any 'rigid' sense,

as alleged by Professor Frankfort, but was tentative and provisional, and has since been modified in various later studies, as Professor Snaith has recognized in his valuable criticism of *Myth and Ritual* in his book *The Jewish New Year Festival*. The attempt to show a generic similarity underlying the culture patterns of the ancient Near East was never intended to deny or preclude the existence of those important differences which Professor Frankfort has described in his book *Kingship and the Gods*.

There is not space to enter into the discussion of the many controversial points raised by the Frazer lecture, but two final points may be mentioned. Professor Frankfort, with the object of refuting the suggestion that the sacred marriage formed part of the pattern of Egyptian religion, asserts that Osiris never celebrated the ritual of the sacred marriage. None of the 'guilty men' ever said he did, but on pp. 34-7 of *Myth and Ritual* Professor Blackman puts forward convincing evidence of the existence of a sacred marriage, viz. that between Horus and Hathor, as part of the ritual pattern of Egyptian religion. The other point concerns Professor Frankfort's denial that any of the main features of the Babylonian celebration are to be found in Old Testament sources. No one doubts that the pattern of Hebrew religion as we find it now in the Old Testament is the result of a profound transformation resulting from the impact of the experience of the eighth-century prophets and their successors upon the contemporary pattern of religion which confronted them. But this contemporary pattern contained far more than what Professor Frankfort calls 'secondary' features. Hosea's polemic against the current conceptions of the source of fertility is meaningless unless it implies the idea of the sacred marriage as its target. The existence in Israel of *qedeshoth*, or sacred prostitutes, is generally acknowledged, and that institution is the accompaniment and outcome of the ritual of the sacred marriage. The myth of the slaying of the dragon is frequently found in Hebrew poetry, with Jahveh as the hero of the myth. Professor Frankfort himself accepts Hans Schmidt's evidence for the existence in Israel of a ritual enthronement of Jahveh. These are not 'secondary' features, and to recognize their presence in Hebrew religion is only to enhance the achievements of the prophets in transforming and spiritualizing them and so bringing to birth the noblest religion the ancient world was to know. But no doubt the Old Testament experts will in due course join issue with Professor Frankfort, and the swing of the pendulum will right itself. Meanwhile we can be very grateful to Professor Frankfort for his brilliant exposition of the specific differences between the culture areas in question without denying the possibility of a generic similarity underlying them.

S. H. HOOKE

**Prehistoric India.** By Stuart Piggott. London (Pelican), 1950.

Pp. 293, 8 plates, 32 text figs. Price 2s. 6d.

**I45** A book of any kind on Indian prehistory, let alone a good one such as this one is, is not only an event but an event that is almost unique. As Professor Piggott states in his introduction, it is high time that there was a stock-taking of our knowledge, which has increased immeasurably in the last twenty-five years; for it must be admitted that hitherto works on Indian archaeology have dealt mainly with art and literature with the excuse that there was little else to write about.

The book opens, after a review of the recent work which has made the necessary material available, with a chapter on the Indian Stone Age. This period still remains rather unco-ordinated, but stone industries linked with glacial and inter-glacial, pluvial and dry periods are at last falling into tentative chronological groupings. Actually there is more material recorded showing the transition of the Upper Palaeolithic into the Mesolithic than is suggested here; for example, Cammiade's finds at Nandi Kanama and, more especially, Todd's at Khandivli showing a blade and burin industry developing into a microlithic phase.

The background of early Western Asiatic cultures is well and firmly established and the reader is given a clear introduction to the cultural succession in Mesopotamia and at the key sites of Iran—Sialk, Giyan and Hissar. In connexion with Hissar III, Piggott states (p. 63, note) that he is in agreement with Schaeffer's date of 2300-2100 B.C. for this cultural period. While this date is preferable to the earlier one proposed by McCown, there are so many



features of the Hissar III cultures that cannot be placed earlier than 2100 and in some cases than 2000 B.C., that even this date might be brought down to a starting point of 2100 B.C.

The peasant industries of the early inhabitants of Makran, Baluchistan and the Indus Valley are very well described; in fact Piggott can rightly claim to have produced the earliest comprehensive co-ordinated survey of these sites, and there is a clear exposition of the complex problem of the inter-relationship of Kulli, Mehri, Nal and Nundara with one another and with Amri and the Harappa Culture. The relationship between Nal and Periano Ghundai bears another interpretation. The three pots which are definitely of Rana Ghundai IIIc type come from Area D, the top 'burnt' portion of the Sohr Damb mound at Nal. This destruction could be added to the others, c. 1800 B.C., in the Nal column of Chronological Table II. While the Nal culture cemetery is more recent than the rooms in Area A, it must from the recorded position of Nal types in the Indus valley be earlier than R.G.IIIc, which as Loralai IV I place as starting about 2250 B.C. Even if this cemetery indicates a very late phase of the culture which produced the Nal-Nundara type of pottery, it could hardly be later than c. 1800 B.C., when the final settlement was destroyed.

Other points also call for comment. The red-ware and buff-ware classification does not hold good for the sites round Loralai at every stage of occupation. The pottery in Brigadier Ross's Bull Level, Piggott's R.G.II and my Loralai II, is buff ware, as also are sherds from Rana Ghundai which Miss de Cardi, by comparison with her own specimens, states to be Quetta Ware. Sherds of bichrome, scarlet and black, painted pottery, R.G.IIIa, have a dark red slip, again on buff ware. Red pottery with black decoration of R.G.III b and c is identical with similar pots from Periano Ghundai. The Loralai region is marginal, the earlier buff pottery coming *via* Quetta from the south and the later red pottery coming *via* Periano from the north. It is for consideration also whether the grey pottery of Quetta with looped border and patterns having ragged edges is not linked with similar plates from the Zhob, and may not date considerably later than the dark-brown-on-buff Quetta ware.

The Harappa civilization is fully and admirably described. Nothing has been omitted, and a clear picture is drawn of this essentially mercantile people, showing them to be at the same time technically and administratively competent and mature, but singularly lacking in that urge to experiment which produces progress and change. The case for an early date for the Harappa statuettes, based on the early authenticated use of metal and shell attachments and inlays and the artistic excellence of the dancing girl figure from Mohenjo-daro, is convincingly argued. A very important and I believe correct suggestion is put forward that the earlier evidence for contacts with India found in Mesopotamia is of objects derived from intercourse with Makran and South Baluchistan, and that trade relations with the Indus were not established until five hundred years later (pp. 117f.).

One might have thought that the source of the stone 'amazonite,' found at Mohenjo-daro, had been finally settled, but apparently this is not the case. On the strength of a verbal communication from Mr. C. S. Middlemiss, the source of this stone was given in Sir John Marshall's *Mohenjo-daro and the Indus Civilization* (p. 546) as Doda-betta in the Nilgiris and, doubtfully, Chishoti in Kashmir; enquiry from the Geological Survey of India failed to confirm either of these sources. All this is however quite beside the point. Amazonite was found by Bruce Foote and subsequently by Dr. Sankalia in the bed of the Sabarmati in Gujerat. Gujerat is the prime region whence India has always derived semi-precious stones; the carnelian, onyx and agate found at Indus sites came from this area, and it was here also that in the search for such stones, amazonite was discovered.

The site of Rangpur, which has always been suspect, though of great antiquity, has at last been shown by the work of Dr. Dikshir not to contain evidence of Harappan occupation; it must be dropped from the list of such sites and cease to be quoted as a southerly extension of this culture.

I feel that the Harappa contacts, put forward admittedly in very tentative fashion, with Hissar III and Anau III (p. 209), do not amount to much. The metal types of these last—daggers and spearheads with midribs or lozenge section, single-loop and double-spiral

pins, socketed axes and spearheads and compartmented seals are all characteristic of new forms, emerging in my opinion about 2100 B.C., whose sole connexion with India is due to the impact of westerners from about 1800 B.C. onward.

A more convincing picture than any I have yet read is given of the coming of the Aryans. The whole set-up is identical with that of the heroic age of Cuchulain and Fian in Ireland—the family bards, the cattle-raiding, the gaming and feasting, the clan rivalry and the bardic cursing of enemies be they Arya or Dasa alike. Professor Piggott uses his literary sources with admirable objectivity, gathering from them a convincing description of the equipment and mode of life of these newcomers. They show themselves materially as squalid barbarians armed with the best weapons of their time; in their outlook however they are romantic, tough and adventurous—qualities that the urban, stodgy, mercantile communities of the Indus could not match.

As Piggott only takes us down to c. 1000 B.C., although there are yet 750 years which are to all intents prehistoric, we get but little information about the south. Though he mentions the Brahuis with their Dravidian type of tongue 'islanded' in Baluchistan, we hear but little of the Dravidians. Suffice it to say that had the whole of the north been populated to a great extent by Dravidians, we should have had far greater evidence of the fact than the language of the Brahuis, and it is increasingly more apparent that in the same way as there was a 'Coming of the Aryans,' so too was there a similar and later 'Coming of the Dravidians.'

These notes give but a small idea of the immense amount of material which has been collated and set out as a coherent whole in this excellent book, which, it is hoped, will one day reappear in the more enduring and imposing format it so richly deserves. At the same time one hopes that its present cheap form will make much more widely spread the interest in what must now be recognized as one of the most important areas of the Ancient East.

D. H. GORDON

**Everyday Life in Ancient India.** By Padmini Sengupta. O.U.P., 1950. Pp. 202, illus. Price 8s. 6d.

**146** This book is an attempt to survey the everyday life of India from prehistoric times to the mediæval period—roughly, for the author sets herself no strict chronological limits. It is directed largely at an Indian public but can be enjoyed by the English reader who has a little general knowledge of the main outlines of Indian history. It makes no pretence at scholarship or originality, but describes simply and pleasantly such things as clothes, jewellery, universities, the position of women and so on. At times it is a little too much the primer, especially in the sections on religion and literature. Still, such books on Indian life for the general public are too few for any stern criticism. On the whole this is a pleasant and unpretentious little book.

DOUGLAS BARRETT

**The Book of the Zodiac.** Translated by E. S. Drower. *Oriental Translation Fund*, Vol. XXXV. London (R. Asiat. Soc.), 1949. Pp. 218. English translation, and 289 Mandæan text, with 2 tables.

**147** Lady Drower's translation is based on three twelfth and fourteenth-century copies of a Mandæan manuscript miscellany of astrology and omens, themselves translations, sometimes at second hand, of Arabic, Greek, Persian and Pahlavi originals, whose writers probably drew on still older material. None were Mandæans by religion. Hence the texts are of no help to students of Gnosticism. It is useful, however, to have access to ancient compilations reflecting the astrological beliefs of the various Jewish and Moslem communities inhabiting the land in which astronomy had its beginnings as a science, incidentally demonstrating the cultural gulf between the instructed minority and the superstitious masses characteristic of ancient civilizations but so often forgotten. Some of the planetary omens (see *The Book of the Stars*, pp. 56ff.) throw sidelights on the early Mesopotamian habit of stressing the calendrical importance in the life of the people of observed astronomical events controlling the seasons—and hence the operations of agriculture—by presenting at the relevant seasonal festivals religious dramatic performances in



which the actors in the masques and pantomimes, allegorically picturing cosmic events, took the parts of the heavenly bodies and thus, in their be-masked and emblem-accounted human guise, became identified, as divine personages, in the popular imagination, with the Sun, Moon and Stars themselves. These particular omens appear to link the pantomime characters of the planets with their actual positions in the Zodiac and thus to have suggested their characteristic 'influence' on people 'born under their star.' Other portents or omens are no more wide of the mark than our contemporary countryman's weather prognostications founded on observation and an unstatistical averaging of accompanying phenomena. Students of the origins and history of Asiatic necromancy, likewise, should be grateful to the Fund for having made the publication of Lady Drower's work possible. V. C. C. COLLUM

**The Children of Hari: A Study of the Nimar Balahis in the Central Provinces of India.** By Stephen Fuchs. Wiener Beiträge zur Kulturgeschichte und Linguistik, Vol. VIII. Vienna (Herold), 1950. Pp. xviii, 463, 64 illus.

**148** The Balahis of the Central Provinces are an 'untouchable' Hindu caste, formerly weavers and now agricultural labourers and small tenant farmers. Father Fuchs' study shows how valuable can be the ethnographic description of a scholar who, though untrained in the theory of social anthropology, has spent many years in intimate contact with a pre-literate people, speaking their language and closely observing their customs. His book is one of the few full accounts yet produced of a single untouchable caste.

It is a lively, readable book. Many anecdotes and case histories testify to the author's sympathetic understanding of his people. The accounts of excommunications by caste assemblies, of attempts by caste elders to reform their customs in accordance with high-caste Hindu practices and of the settlement of disputes within the caste provide material of a kind which, with the decline of the caste system, is now unobtainable in many parts of India. This is also the kind of material most needed: our understanding of caste can now increase only through detailed studies each confined to a single small area.

The book is written as a catalogue of customs, not as the account of a social system. But the material is so rich that the reader can construct from it a fairly adequate account of the internal structure of this caste. Some elementary facts are missing. It is possible, for example, to piece together from Parts II and III an outline of the Balahi kinship system. There is information on social relations within the patrilineal, patrilocal extended family; on marriage-arrangement and the payment of bridewealth in cash; on divorce; on inheritance; and on the splitting of the dwelling group. But there is no reference to the part played by matrilineal kinship ties in inter-village relations, and there is no list of kinship terms.

Again, we are told that the Balahis are divided into patrilineal exogamous clans; that the Balahis of one village frequently belong to a single clan; that they live in separate quarters removed from the high-caste Hindu part of the village; and that they have certain kinds of social relations with certain other higher and lower castes. But we are not told the number and size of the clans nor the size of a typical village. There is no list of the other castes in the village, and we never see how the Balahis fit into the total caste hierarchy. Balahi occupations are given only in Part V under 'Material Culture,' and are not related to the form of their kinship system nor to the economic system as a whole. Similarly, the long section on Balahi religion, though it gives useful material on the way in which gods of the high-caste pantheon are incorporated into Balahi ritual beliefs, fails to relate ritual institutions to the internal structure of the caste.

Nevertheless, this is a painstaking, unprejudiced account. It surpasses the standards of much Indian ethnography. Throughout, the author confines himself to his own field material and is not led into conjectural history by the theories of the Viennese school of anthropologists with whom he has affiliations.

KATHLEEN GOUGH

**Naga Path.** By Ursula Graham Bower. London (Murray), 1950. Pp. x, 260, 22 plates, 2 maps. Price 16s.

**149** The Zemi Nagas of North Cachar, living on the very steep Barail range and practising *jhum* cultivation, needed a large area to support them. Each community therefore had a number of village sites, round which it moved in a regular cycle over a long period of years. The Administration did not understand this and allowed immigrant Kukis to settle on any land not actually occupied by the Zemi at the time. The consequence was that the country was over-populated and always on the verge of famine. When Miss Bower arrived among them in 1939 she found that, as a consequence of this and other unfortunate incidents, the Zemi were suspicious and disaffected. She set herself to remove the barrier by making friends and by bringing medical aid, and had made great progress when the Japanese reached the fringes of her area. She then organized a guerrilla intelligence service. When the war was over the barrier was gone.

The book is written for the general reader, and detailed treatment of ethnographical matters is not to be expected. Nevertheless there is much of great interest: the system of headmen descended from the original founders of village sites; accounts of ceremonies, especially the annual expulsion from the village of the spirits of the dead; the reactions of the relatives, headmen and other villagers after a case of attempted rape. Above all Miss Bower writes vividly and makes it easy to understand why the Nagas and their hills fascinate all who know them. B. A. L. CRANSTONE

## CORRESPONDENCE

**Social Anthropology: Past and Present.** Cf. MAN, 1950, 198 and 254; also 1950, 271 and 1951, 33-35, 78 and 120

**150** SIR.—The difference between Professors Evans-Pritchard and Forde is the difference between scientific exposition and wishful thinking—a position neatly summed up by Tyndall in his Liverpool address of 16 September, 1870, when he said: 'The scientist's business is not with the possible but the actual—not with a world which might be, but with a world that is.' The point of issue is: are there any social laws? So far as I am aware, from extensive reading and much work in the field, there are none, unless it be one that I discovered myself, namely that in a society which has an imposed or acquired religion, the keywords are not indigenous. But then this enunciation is more a law of culture change than a social law. Professor Evans-Pritchard and Professor Lowie have stated the present position. Professor Forde is either ignorant of the fact that there are others who hold this view or else he has wilfully refrained from mentioning that Professor Evans-Pritchard is in the good company of such anthropologists as Kroeber and Boas, and of the eminent historian Fisher. I propose to quote

from these to see what Professor Forde's reply will be. Kroeber writes (*Configurations of Culture Growth* (1944), p. 761):

'In reviewing the ground covered . . . I see no evidence of any true law in the phenomena dealt with; nothing cyclical, regularly repetitive, or necessary. There is nothing to show either that every culture must develop patterns within which a florescence of quality is possible, or that, having once so flowered, it must wither without a chance of revival. After all, cultures merge into one another, and so cannot have the individual entity of higher organisms. Nor is there any reason to believe that historians are wrong in having consistently refused to believe, or at least in doubting, the presence of strict universals in the record of human and social events. Experience attests that universals are to be found in abstracted properties or processes, not in specific phenomena. And in the light of such physical and biological constants or regular properties as have as yet been discovered, the facts of history and culture appear not even phenomena so much as epiphenomena.'

I would go further and say that culture and society are the arbitrary



manifestations of man's free will and that where free will acts there can be no laws, the terms being mutually contradictory and exclusive.

Boas writes (*General Anthropology* (1938), p. 5):

'If regular cultural sequences could be found, these would represent an orderly historical cycle. If laws of sequence and of social dynamics could be found, these would be sociological laws. It is one of the important tasks of anthropology to determine how far such regular sequences and sociological laws exist. When this task has been achieved the principal problem remains, that of understanding culture as a whole. Neither history nor sociological laws are of considerable help in its solution. History may tell us the sources from which bodily form, customs and beliefs have been derived, but it does not convey any information regarding the way in which a people will behave owing to the transmitted characteristics. Sociology may teach us the morphology and general dynamics of society; it will give us only a partial insight into this complex interaction of forces, so that it is not possible to predict the behaviour resulting from the historical events that made the people what they are. This problem is essentially a psychological one and beset with all the difficulties inherent in the investigation of complex mental phenomena of the lives of individuals.'

Now the aim and object of science is to formulate laws that can be used predictably. Boas makes it clear that such social laws are not discernible when he writes that sociology 'will give us only a partial insight into the complex interaction of forces, so that it is not possible to predict the behaviour resulting from the historical events that made the people what they are.' Professor Evans-Pritchard urges that the historical approach is the sheet anchor in the study of social anthropology and that this approach affords no prospect of being scientific in the sense of discovering predictable laws of human behaviour; Professor Forde claims that there are laws to be found. H. A. L. Fisher, writing of history, says (*A History of Europe* (1943), p. v):

'One intellectual excitement has, however, been denied me. Men wiser and more learned than I have discerned in history a plot, a rhythm, a predetermined pattern. These harmonies are concealed from me. I can see only one emergency following upon another as wave follows upon wave, only one great fact with respect to which, since it is unique, there can be no generalizations, only one safe rule for the historian: that he should recognize in the development of human destinies the play of the contingent and the unforeseen.'

None of these authorities sees any evidence of social laws in human society.

The easiest way to convince such men of their ignorance or blindness is to confront them with social laws. Does Professor Forde do so? He would if he could, but he cannot, so he relies on what Tyndall objected to—'a might-be.' 'If,' says Professor Forde, 'in the field of social science a verified general statement to the effect that  $\langle (or) \rangle X \supset \langle (or) \rangle Y$  can be made with reference to distinct attributes of associated patterns of behaviour, we have the equivalents of 'laws' in physical and biological science, i.e. abstract statements that have not been falsified to date by examinations in the relevant field of controlled human observation.'

Now what does all this verbiage amount to? If... says the Professor, then we have the equivalents of laws. Either there are social laws or there are not. 'Ifs' and 'equivalents' are no substitutes. Enunciate a social law and let there be no argument by analogy from physical and biological laws, for that is what much of Professor Forde's letter consists of.

What has become clear is that as culture is learned behaviour and behaviour is learnt *via* language, and as it is in this realm only that man can exercise free will, it follows that the 'laws' of culture are the 'laws' of free will. But 'law' and 'free will' are mutually exclusive. Consequently with free will exercised *via* language the search for laws in a free-will continuum is illusory.

History alone will provide the lamp that will illumine the origin and growth of culture; equivalents of biological and physical laws have no place in culture.

Anthropologists will be amused to read that they 'have quite

generally grasped all this [whatever this connotes] intuitively even when it has not been formulated.'

Better to follow in the search for social laws a leader with the historical lamp illuminating comparative culture than to follow the vagaries of an intuitive guide.

M. D. W. JEFFREYS

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**Race.** Cf. MAN, 1950, 220 and 1951, 28–32, 93–96 and 122

**151** SIR.—As anthropologists not only working but also having our homes in a racially mixed community we welcome the suggestion of a statement on 'Race' by a body such as U.N.E.S.C.O., although like most of our European colleagues we found the original statement unsatisfactory. In view of your note (MAN, 1951, 32) therefore we forward some points for the consideration of the new panel.

(1). Any statement is bound to have political repercussions if it is to be of any use at all; it must therefore be couched in plain, unambiguous language which the least scholarly of legislators can understand and it must also be free from generalizations which would tend to lower its value in the eyes of the critical and impair its usefulness for those without anthropological training.

(2). Paragraph 9 of the Ashley Montagu Statement seems to us a most unfortunate one. In the long run, reaching back as far as the Upper Palaeolithic (the 'Middle Stone Age' of Southern Africa), it may be true to say that the general achievement of all 'ethnic groups' is the same. The modern statesman, however, deals at the most in decades and cannot take into consideration a process which may last for millennia.

One of us has been in close contact with the Southern Bantu for nearly 40 years and is of the opinion that under favourable conditions and given ample opportunities an outstanding Ndebele or Shona man is well able to attain a high standard of education and culture according to European standards. On the other hand such men are very few and whilst the level of literacy amongst the Bantu of Southern Rhodesia has risen almost miraculously during the past 20 years, yet the level of intelligence is still depressed by a complex of fears which may well take generations to eradicate. These fears are not, in our experience, the result of White conquest and domination but arise in everyday life where fear of the unknown evil influences in the spirit world and fear of other members of the family are felt side by side with fear of external enemies. The domination of the earlier inhabitants by Shona invaders and of these latter by the Ndebele has formed a pattern of servitude which, combined with the Bantu fear of the unknown, is a very potent factor in retarding their mental and material culture so that even today it is often no higher than that of our Early Iron Age ancestors in Europe 2,500 years ago.<sup>1</sup>

(3). We fully agree with paragraph 12 of the Ashley Montagu Statement (unfortunately omitted from the shorter press release). The personality and character of some of our Bantu friends surpass those of many of our acquaintances who have had far better educational opportunities. At the risk of appearing snobbish we must add that in many cases of which we have personal knowledge personality and character appear at their highest level amongst those descended from the old ruling houses, although some of these houses have been submerged for at least five generations. We make this as a point of interest and not as a scientific fact, for our data are too scanty for generalization.

(4). The question of miscegenation is one which is of great interest in the mixed community in which we live (see paragraph 13 of the Ashley Montagu Statement). We realize that many of the disharmonies and degeneracies alleged against the Coloured (i.e. half-caste) population in Southern Africa are the result of social conditions, but we feel bound to draw attention to the question of sicklaemia amongst Indian and Coloured people, a condition which appears to arise purely from past miscegenation (Elsdon-Dew, *S. Afr. J. Sci.*, Vol. XLVII (1951), pp. 176–9). It does seem that European-Bantu crossing produces a physically weaker stock.

(5). Paragraph 14 of the Ashley Montagu Statement has already been criticized by many more distinguished than ourselves, yet we cannot but feel that there is much truth in it. Very many of our Rhodesian racial problems are basically social ones which are



aggravated by obvious physical and physiological differences. We therefore welcome the separation of U.N.E.S.C.O.'s race studies into physiological and social ones. If the emotional basis of racialism can be removed there is some chance of social inequalities being in time reduced.

In view of the misconceptions existing about the political affiliations of the Colony of which we have the honour to be citizens we would add that Southern Rhodesia is a Self-Governing Colony and is not part of the Union of South Africa.

*c/o National Museum of Southern Rhodesia,  
Bulawayo*

NEVILLE JONES  
ROGER SUMMERS

#### Notes

<sup>1</sup>Literacy we know, but what is intelligence? And what was the mental culture of our Early Iron Age ancestors?—ED.

<sup>2</sup>But surely the gravamen of the criticisms made of the Ashley Montagu Statement has been precisely that it failed to distinguish clearly between the physical and social aspects of race, and that distinguished sociologists and historians were assembled to determine biological facts. The criticism of paragraph 14 has been concentrated upon its attempt to represent race, and not merely racialism, as a myth.

U.N.E.S.C.O. decided, wisely and promptly, to convene a new panel to isolate the established biological facts of race, and we may hope that an effort will later be made to obtain a consensus of opinion upon the psychological and social phenomena of race (it would perhaps be too much to refer to 'established facts' in this connexion). Our correspondents' valuable and admirably balanced observations may in part be more germane to these later deliberations.—ED.

**The Framework of Prehistory.** Cf. MAN, 1951, 64 and 119

**152** SIR,—Dr. Daniel's plea for an objective chronological framework within which we may set the sequence of cultures in pre-literate Europe involves me (though indirectly) in *limine*, but it is not for this reason that I write partly to support and partly to criticize his thesis. All of us who have tried to write prehistory in terms which avoided archaeological jargon have I think experienced the inadequacy of the Stone-Bronze-Iron classification, and when faced with the problem of whether makers of Beakers in Britain were Bronze Age or why the farmers who whetted iron knives at Plumpton Plain were nevertheless Late Bronze Age, have either hedged, or, if more courageous, admitted defeat. The time is obviously ripe for a reconsideration of terms.

Daniel has put up a case, with alternative systems A and B, for a set of 'period' divisions from 3000 B.C. to A.D. 500, and related only to Europe. He has invited criticism, and it is convenient to start with this, before attempting any alternative ideas. My objections, then, to his scheme are as follows:

(a) It is limited in time (3000 B.C.—A.D. 500). If therefore we wish to relate this to an earlier or later time scale (Mesolithic into Neolithic: Dark Ages into Medieval) we would find ourselves in difficulties, writing 'Bone types originating in Mesolithic (or Maglemose, or Boreal) times appear at Skara Brae in Periods 5/6 (or IV/V—or would it be Barbarian IV/V?)' or 'The typologically earlier hanging bowls may belong to Period 14 (XII), but the designs on certain escutcheons are paralleled in the Book of Durrow of the late seventh century A.D.' This is clearly unsatisfactory.

(b) It is limited in space to Europe. Here again we would find ourselves in hopeless confusion if we were to try to relate European to Asiatic events, for instance: 'The South Italian painted pottery of Periods 1-3 (I-III) is stylistically comparable with some from Macedonian sites probably of the late fourth millennium ('pre-I?') and indeed that of the Halaf period,' or 'The use of the war chariot, known in the Hittite Empire from c. 1500 to 1200 B.C. is not attested in Central and Northern Europe before Period IX.' Again we are in the awkward position of describing a single group of facts in two systems of time-reckoning.

If therefore we are to use a system of objective chronology, why

not the universal scale of millennia, divided, I would suggest, into thirds of Early, Middle and Late? This would avoid hard-and-fast divisions between 'periods,' for 'Middle Second Millennium' would not give the misleading impression that one regarded the phase as strictly from 1666-1333 B.C. If more precise dates could be given ('early twelfth century B.C.' or 'c. 1500-1450'), they would fit within the millennial framework.

There still remains a problem. A convenient usage has arisen whereby for instance 'Mesolithic' can be employed to denote the North European hunter-fisher traditions, irrespective of their chronological horizon (cf. Gjessing's use of the phrase 'Circumpolar Stone Age' to cover a set of traditions extending chronologically from Boreal times to the present day. Some such terminology must be available, but whether the old technological stages are to be used as labels for this purpose is a matter for further discussion, to which I hope that anthropologists will contribute their views.

STUART PIGGOTT,  
*University of Edinburgh*

Professor of Prehistoric Archaeology

**153** SIR,—Dr. Daniel's proposed chronological framework would be a most welcome step towards clarification of the 'anarchy of prehistorical nomenclature.' It is not enough, though of the greatest importance, to free the conception of 'periods' from cultural associations; but any system adopted should also provide a basis for a painless and progressive transition into the historical time scale. Dr. Daniel takes 3000 B.C. as the fixed point for the beginning of his Period I; this date, however important in prehistory, has value only relative to that other arbitrary fixed point adopted by Western historians, the birth of Christ. Would not a system of periods reckoning from the same arbitrary but venerable fixed point be preferable? This would make easier on the one hand an extension of the periods backwards and forwards as required, and on the other hand progressive adoption of a more precise scale. Multiplying the period number by three would then give the upper limit in terms of centuries (modification of Dr. Daniel's Scheme B) or division by four in terms of millennia (Scheme A). Personally I prefer Scheme B as more closely integrated with the historical system. However imprecise our knowledge of cultural horizons contained within them, the periods themselves should be limited by exact dates. The framework if it is to be more than the old culturally defined periods must be rigid. Regionally, progression from proto-historic periods to historical years (through the gradation of Professor Hawkes's system of nomenclature) would be easily effected within this framework as and when accurate data became available.

*Oxford*

N. K. SANDARS

**The Keilor Skulls.** Cf. MAN, 1949, 146 and also 1946, 94

**154** SIR,—In H. V. V. Noone's article on 'Some Implements of the Australian Aborigines with European Parallels' the statement is made that the early arrival of man in Australia 'is said to be attested by a recent find of two fossil human skulls at Keilor, near Melbourne.' In fact, only one fossil human skull from this site has received scientific attention. Noone's statement is probably based on D. J. Mahony's article, 'The Problem of the Antiquity of Man in Australia,' in *Mem. Nat. Mus.*, No. 13, 1943, which records that two skulls were found at a depth of nineteen feet. This article later contains the statement that the second skull and most of the other bones had not been received by the Museum, of which the late D. J. Mahony was then Director. The second skull has never been received by the Museum, and enquiry amongst those scientists most closely associated with the discoveries at Keilor has failed to elicit its whereabouts. Its existence is, therefore, problematical.

It also appears likely that the Keilor skull is not as ancient as was at first thought. Work is in progress in this Museum and in the University of Melbourne on the whole problem of the discovery, authenticity, characteristics and age of the specimen, and it is hoped that a better definition of the scientific status and meaning of this key relic will emerge in due course.

*National Museum of Victoria*

DONALD J. TUGBY









(a) THE FRIEZE AT THE RUINS OF MEALLAU



(b) THE ROCK-CUT FRIEZE AT DARHO CAULOS

**TWO ERITREAN ROCK SCULPTURES PROBABLY OF COPTIC ORIGIN**

*From watercolour drawings by Mrs. F. G. Drew*



# TWO ERITREAN ROCK SCULPTURES PROBABLY OF COPTIC ORIGIN\*

by

SANNIE DREW

## Introduction

**155** The long narrow plateau of Eritrea, 2,300 metres above sea level, is a continuation of the Ethiopian highlands. From Asmara the great Italian motor road runs almost due south for 1,100 kilometres to Addis Ababa. For half of its length it is built on the watershed between the Nile and the Red Sea. The gorges to the east are short

village of Emba Cilai and mountainous country stretching to Ethiopia.

On a smooth sandstone rock face is the large carving, depicting a martial scene, which is illustrated in Plate Ga and fig. 1. It measures 3.30 metres wide, 1.52 metres from top to bottom and 1.22 metres from the ground. The rock is grey with a black patina and the figures have been hammered out in sunk relief so as to show grey against the black surface; they have the appearance of considerable age, and red and yellow lichen has grown in vivid patches. Of the nine figures eight are male and one probably female. Four of them are almost life-size, two carrying short heavy lances and one a straight-bladed sword; the third large figure may possibly be a female who is being carried. Of the five smaller figures, two are perhaps represented as tied to stakes. The two figures on the right seem to be flying through the air, the larger man piercing the head of one of the staked figures with a sword-like object. All the figures seem to be clothed. The large warriors have a short, jutting hairdress and are broad-shouldered and much waisted. On the left is a big V symbol, and below is part of a short-horned ox.



FIG. 1. THE FRIEZE AT MEALLAI  
*For comparison with Plate Ga*

and very precipitous, while to the west they are long and flow through much mountainous country. In some places the watershed is only 50 metres wide.

At Adi Caiah, 120 kilometres from Asmara, this southern road starts to cross the old trade routes from the Gulf of Zula and the ruined port of Adulis to Axum, the ancient capital of Abyssinia.

## *The Rock Carving at the Ruins of Meallai*

To reach the site of Meallai, one follows a rough road running west from Adi Caiah to Coatit for about five kilometres, then strikes due south at a low cactus hedge across rough ground and fields for two kilometres. Here a stone buttress rises 30 metres above the plateau; the country drops in huge terraces to the Hazimo plain in the distance and there is a magnificent view south over the

\* With Plate G and two text figures



FIG. 2. THE FRIEZE AT DARHO CAULOS  
*For comparison with Plate Gb (left-hand portion)*

In front of the frieze there are two or three flat-topped rocks about 90 centimetres wide, and 10 metres in front and a little lower down the hill is a big regular-shaped rock with absolutely flat top 2.50 metres wide. Seven metres to the north and high on the wall there is a roughly chipped quadruped about 20 centimetres long.



We climbed on to the shelf above the rock and found heaps of stones and foundations of buildings with well laid blocks of granite. The Italians had dug a hole in one place and I picked a round white bead off the heap of exposed earth. The natives, who are Copts, said that many such were turned up in ploughing. The old men said that there were the ruins of nine towns just there. The name Meallaui meant 'infinite (number of people)' in some language other than Tigriniyan. It had been built, they said, by King Meron, the father of Akelecion and Teclecion, from whom they were all descended (the district was called Akele Guzai). They could give no idea of the number of generations since King Meron.

### *The Rock Carving at Darho Caulos*

The village of Darho Caulos lies nine kilometres south-west of Asmara. A rough road leads past the old prison camp of Sembel (four kilometres) to a small farm, four kilometres further on, which was abandoned three years ago for fear of bandits. Below the farm is a little spring and a path leads one north-west, by a half-hour's walk, up a small valley wild with roses and asparagus fern to a cave 2 metres high, 12 metres wide and about 30 metres deep, which is known simply as Ba'ati, 'the cave.' The outer chamber is at present used for cattle, the inner has several passages all appearing to have been worked with a

*scalpello di dente di cane*. The soil is clay and sedimentary sandstone.

There are no drawings or carvings in the cave itself, but at the entrance about 1.20 metres from the ground there is a group of 27 naked figures carved in bold relief in the red sandstone (Plate Gb and fig. 2). The carved surface is 1.84 metres wide and 1 metre high. The figures vary from 25 to 50 centimetres in height; the forms are well rounded and all are female except one which is sexless. Each of the female forms has a heavy mop of hair, and seven of them have flat, splayed and deeply scored fingers and toes. Deep holes mark navel and sexual organs. All the figures have been wantonly defaced, but where the surface of the rock is untouched it is smooth, almost polished, and seems to have been carved with a sharp implement. Close examination after washing of some of the undamaged portions of rock brought to light fine patterns or lines. On the right arm of the figure third from the left there is a Coptic cross, and a sort of cross also appears on the stomach. Around the chest of the smallest figure is a spotted band, and another figure on the right has a sort of necklace.

Below the carving to the right there is a broken inscription in Ge'ez, of which I have taken a squeeze. To the left of this are some roughly incised drawings.

Local tradition says that the cave was the dwelling of a hermit and that the spring is sacred water and cures ophthalmia.

## NATIVE COPPER IN RELATION TO PREHISTORY\*

by

H. H. COGLAN, A.M.I.MECH.E., F.S.A.

**156** In a recent paper given before the British Association, Adrian Digby<sup>1</sup> has pointed to the tendency of those whose work lies in the field of material culture to 'specialize more and more in what may be termed the comparative anatomy of artifacts without paying sufficient attention to how they are used and how they work.' Again, he observed that, if we do not know all the details and demands of the technical processes of a people, we cannot reach a true understanding of their social organization. These remarks have a most appropriate bearing when we consider the important subject of prehistoric metallurgy. Much study has been devoted to the metals in antiquity, but there still remains an enormous amount to be done. It is enough to say that in all probability, in the case of 90 per cent. of the many thousands of metal specimens contained in our museums, we do not know the true composition of the metal, or by what particular process the artifact was made. How, then, can we have any adequate knowledge of the rise, or form an accurate idea of the technical progress, of the early metal-working cultures?

\*The substance of a paper read before the British Association, Section H, Birmingham, 1950

It is indeed unfortunate that we cannot define the place, and importance, of native copper in prehistory. We can only say that it is now usually accepted that the native metal was the first copper used, and that it was still used in Europe so late as the developed Bronze Age—the long intervening period is still a blank. This gap in the archaeological and technological record is the more serious because, while there may be argument about the quantity of copper used during the early periods, there can be no question of the importance of a discovery which, although it was not a metallurgical one, did in time lead to the dawn of a true metallurgy. Of course, there is no need to stress the importance and influence of metallurgy upon the progress of the human race. An important problem in the study of the development of technology and applied science is the extent to which native copper was used in prehistoric times; and of how its use in its natural form, and as a melted metal, preceded the knowledge of how to smelt copper from the ores.

In the Old World native copper has a wide distribution; even today we have secured specimens from the following countries, and certainly much remains unrecorded—England (Cornwall), Scotland (Renfrew), France,



Hungary, Central Germany, Russia (Urals), Spain, Norway, the Faeroes (Naalsoe), Persia, Anatolia. In America, the very extensive and valuable deposits of native copper from the Lake Superior regions are of course well known. Also, the experienced mining authority Dr. T. A. Rickard considers<sup>2</sup> that the native metal is far more abundantly distributed than is generally supposed. The actual quantity of native metal which was available to prehistoric man is open to question. Without far more evidence it is exceedingly difficult to form any opinion upon this matter; it is significant that there was a substantial amount of native copper available at the surface in Hungary,<sup>3</sup> and in Anatolia it is believed that the Arghana deposits were worked to within recent times. Przeworski<sup>4</sup> appears to be in no doubt as to the early supply of native copper in Anatolia, but a modern report upon the copper resources of that and other countries is much needed. Again, it is important to note Bromhead's statement<sup>5</sup> that even now native copper is abundant in the Urals.

The form in which native copper is found has an important bearing on the use to which it could have been put by a primitive people who lacked the knowledge of how to melt and cast the material. As mineralogical specimens we commonly find the metal in three varieties. First, it is sometimes found in pieces consisting of a great number of small grains or pellets, and also as a sort of spongy growth; obviously such material had to await the knowledge of how to melt it before it could have been made use of. Secondly, the metal has been found in a laminated form, or what may perhaps be described as an arborescent growth. Here, by careful selection of suitable pieces, small awls, pins, and the like could have been made by a grinding or cold-hammering process. Thirdly, we come to what may be called the 'massive' form. Here we find lumps of seemingly solid copper varying in weight from a pound or two up to blocks of copper of great size and weight. Sometimes the copper is not mechanically sound owing to extensive non-metallic inclusions. In the case of this massive copper, it is again difficult to see how prehistoric man could have made use of it prior to the knowledge of annealing, because it is decidedly tough and could not have been cut without adequate tools.

To obtain some idea of the practical working of the massive form of native copper, I carried out various simple forging experiments. Setting out to forge a small chisel, I noticed, during the operation of cutting out a small rod of square section from the nugget, that the copper appeared unusually tough to cut with a steel hacksaw. When cold forging was attempted it was found that the metal was not sufficiently malleable to work. Indeed, its shape could be but little changed by hammering, for under the hammer the metal almost at once became brittle and cracked; not until it had been annealed twice could anything like a chisel edge be attained. These results agreed with experimental work by Philips,<sup>6</sup> who carefully hammered a nugget of Lake Superior copper to try to make from it a copper sheet, but found that a few blows showed a tendency of the copper to crack along the edge as it expanded, so that it required frequent annealing and hammering to make the

sheet. The suspected hardness of massive copper in its unworked condition was confirmed by some figures obtained by Messrs. Alfred Herbert, who kindly examined a nugget of the Lake copper used in the experiments. They reported that 'hardness testing in four places with a 2-mm.-diameter steel ball under 20 kgs. load, on solid regions of the copper, gave an average Brinell hardness number of 78.1. This value corresponds approximately with the "half hard" temper for cold-worked copper of modern origin and commercial purity.'

We can only speculate as to why and in what circumstances heat was first applied to copper. It is, however, certain that the use and value of native copper for practical purposes were very greatly increased when the advantage of raising it to the annealing temperature was discovered. The metal could now be forged in an efficient and practical manner, for when it was found that it was hardening under the hammer, it was easily returned to the soft condition by further annealing. The actual forging could be carried out upon the metal when it was either hot or cold; for ease in handling it is likely that forging was often, probably usually, a process of cold working, coupled of course with intermediate anneals. At this stage in technical development it became possible to forge small lumps of massive native copper, and considerably larger objects such as spears, small celts, chisels, etc., could be made. Owing to internal flaws and inclusions no doubt much copper would not forge to the required size for the objects desired. However, the work of the North American Indians leaves no room for doubt that perfectly practical and satisfactory artifacts were made in this way. As it is a low- or medium-temperature process, it is logical to consider that annealing was discovered before either melting or smelting. Melting was a discovery which would naturally follow on from annealing, for there would have been a natural tendency to increase the furnace temperature in order to see if the greater heat would render the copper even easier to work until in the end a sufficient temperature was attained to melt the metal. To the modern technician all this is very obvious, but it is quite possible that a very considerable time elapsed between the two highly important discoveries just mentioned. Indeed, consideration of the archaeological material available from the early copper-using cultures would indicate that this is so. The use of native copper may then be divided into various phases (naturally having a very different place in the archaeological time scale in different parts of Europe and Asia) which we may summarize as follows:

(i) The earliest use of native copper was carried on with an extremely limited Stone Age technique by which small, specially selected pieces of metal were made into awls, pins, hooks, etc., through cold forging or grinding. The importance and duration of this phase is open to question, and metallographic evidence is much needed in order to establish the true facts.

(ii) Secondly, we find an improved but still very limited technique: it was discovered that the metal could be annealed, and therefore much more easily shaped, after the application of this simple heat treat-



ment. To this phase, which comes under the archaeologist's 'hammered-copper culture,' a considerable body of material may be ascribed on typological grounds. For concrete proof we must again seek the help of the metallurgist.

(iii) Thirdly came the discovery that copper could be melted and cast. This very important phase is marked by the appearance of castings of simple form, such as would be produced from open moulds, or, at most, by simple univalve moulds furnished with a cap or backing. On technological and archaeological grounds we have reason to believe that the simple process of melting preceded the somewhat complex operation of smelting from the ores. Hence, it follows that our earliest cast objects should be of native copper.

It will be noticed that I place the discovery of melting before that of smelting the metals. In 1942, Professor V. G. Childe very clearly stated the four major discoveries as: (i) the malleability of copper; (ii) its fusibility; (iii) the reduction of copper from ores; (iv) alloys. Apart from the fact that it would be strange if the more complex and difficult process of smelting had predated the simple melting operation, there is the point which we have already mentioned, that annealing, which would logically be followed by melting, was known for hundreds of years before the discovery of smelted copper. The archaeological record is terribly incomplete in the matter of scientific examination and analyses of metals. It does, however, give us an indication that the above sequence is a correct one. It is tempting to make the statement that the many small and insignificant objects belonging to the earliest copper-using cultures of Egypt and Western Asia belong to our first and second phases, but it would be more correct to say that they almost certainly do, because unfortunately the great majority of such objects have not even been analysed.

Although the number of specimens of native copper so far examined is small, the evidence which we have would indicate that native copper is remarkable rather for its purity than for any element, or elements, to which we could point as peculiar to the metal. There is no difficulty in distinguishing an artifact of native copper if it belongs to the first phase in the use of the metal, that is, an object produced by cold hammering or grinding, for the metallurgist can always differentiate between a metal which has been melted, and an unmelted native copper which has not been subjected to any form of heat treatment. In our second case, where the native copper has not been melted, but has possibly been annealed a number of times, the identification may not be so simple, for the original structure of the metal may have been lost. Bergsøe<sup>8</sup> has carried out some work on this point, and, as all prehistoric copper which has been melted, or smelted, will contain a certain amount of cuprous oxide, he considers that if a photomicrograph of a section of the metal taken from the surface inwards shows that all the cuprous oxide present lies just under the surface of the metal, the deduction is that the cuprous oxide was formed by the heating-up of the metal during the forging process. If so, the native copper was forged and hammered into shape without having been melted.

The last case, that in which the native copper has been melted and cast, is probably the most important one from a cultural aspect. The problem of identification would be of easy solution if one could take the purity of native copper (or of some key element in it) as a criterion. At the present time it would be unsafe to do so because, in certain circumstances, it appears that an exceptionally pure copper, smelted from an ore such as malachite, may closely approach, or even equal, the purity of the native metal. In treating of American copper Bergsøe considers that one would have to be extremely lucky to find a perfectly pure ore. The well-known Swedish authority, Dr. Oldeberg, has given me his view that, from a theoretical point of view, copper produced by smelting is unlikely to be so pure as native copper. The majority of geologists say that there is hardly any copper ore which will not show traces, or small quantities, of impurities; and as the smelted copper is a reflection of the ore, the copper produced will show such impurities too. As an example of such border-line cases we may mention the analyses of six ancient implements, examined for the Royal Anthropological Institute Committee through the valued collaboration of the Curator of the Pitt Rivers Museum, Oxford, of the Copper Development Association and of the British Non-Ferrous Metals Research Association. These implements were selected because, from their form or dating, it was considered possible that they might be of native copper. The artifacts were: (1) a flat celt from Ireland; (2) a flat celt from Cyprus; (3) a shaft-hole axe-hammer from Pesth, Hungary; (4) another Hungarian shaft-hole axe-hammer; (5) a prehistoric cutting implement from El Amrah, Egypt; and (6) a Second Dynasty cutting implement from Badari, Egypt.

The results obtained from these analyses were interesting and instructive. By addition, the average purity of the six specimens was as high as 99.76 per cent. The celts from Ireland and from Cyprus were exceptionally pure, the Irish copper reaching 99.85 and the Cypriot 99.83 per cent. In the Irish celt the only impurity to which we need point is 0.05 per cent. of antimony, and in the Cypriot metal 0.04 per cent. of arsenic; in either case it is open to doubt if a smelted metal would be so pure. The two axe-hammers were of even greater purity (around 99.93 and 99.92 per cent.), and there is no doubt that the origin of the material was the native-copper deposits of Hungary. The Egyptian implements were not quite so pure, and it may be considered that nickel, 0.03 per cent. in each case, is an unlikely element to appear in the composition of native copper, but it is to be remarked that nickel can appear in small percentages, for it was present in approximate percentages of 0.01 and 0.06 in native coppers from the Lizard, Cornwall.

As Dr. Voce<sup>9</sup> pointed out, it may be inferred that all these artifacts had been prepared either from native copper by melting, or from copper ore by smelting. With such close relationships it follows that a very considerable amount of work, and close collaboration between archaeologists, metallurgists and geologists, will be required to make an impression upon this problem. We must again



stress that the gap in our knowledge concerning native copper is very unfortunate, for it leaves the archaeologist in the unsatisfactory position of being unable properly to define his early copper-using cultures. Also, although much is now known about the later phases of prehistoric metallurgy, we are in a weak position so long as we cannot define the place and importance of the native metal as a background to the evolution of prehistoric metallurgy.

It is hoped that these brief notes may serve to draw attention to the outstanding problems and, now that two committees (the British Association Committee on the Composition of Ancient Metal Objects, and the Royal Anthropological Institute's Ancient Mining and Metallurgy Committee) devoted to research in ancient metallurgy

are in being, the problem of native copper may receive due consideration and study.

#### References

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- <sup>2</sup> *J. Inst. of Metals*, Vol. XLIII (1930), No. 1, pp. 304f.
- <sup>3</sup> Von Cotte, *Über die Erzlagertstätten Ungarns* (1861), and *Beiträgen zur Kenntnis Erzgänge*, Vol. IV (1862), p. 145.
- <sup>4</sup> Przeworski, *Metallindustrie Anatoliens* (1939), p. 90.
- <sup>5</sup> *MAN*, 1948, 3.
- <sup>6</sup> *J. Inst. of Metals*, Vol. XXXIX (1926), No. 2, p. 103.
- <sup>7</sup> V. Gordon Childe, *What Happened in History* (1942), p. 54.
- <sup>8</sup> Bergsöc, *The Gilding Process and the Metallurgy of Copper and Lead among the Pre-Columbian Indians*, Copenhagen, 1936.
- <sup>9</sup> *MAN*, 1948, 17.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**Blood Groups and Anthropology.** Report (with the six following articles) of a Special Meeting, 17 March, 1951, at University College, Gower Street, London

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Since the early years of the century, with the discoveries of Landsteiner, research into the distribution of blood groups has made rapid progress. The results of serological testing of human beings are of obvious importance to anthropologists because of the information they may supply on the history of present peoples, and their genetic and racial affinities. The blood groups are, with very few exceptions, the only common human characters the manner of inheritance of which is precisely known. They are fixed for life from the moment of conception and are probably less affected by natural selection than most other characters. Nine genetically independent blood-group systems are now known. The individual blood groups within most of these systems show marked racial variations in frequency. The blood groups are thus able to provide a valuable basis for a genetical interpretation of human diversities, and the work should be brought into relation with other researches in human morphology, and blood-group observations should be included in all anthropological surveys.

In addition to formal anthropological work, the records of blood transfusion services provide large masses of blood-group data. One of the problems is how to collect, assess and make available to anthropologists the vast and rapidly growing mass of data existing in the form of unpublished records and of publications in a very large range of journals.

In order to try to promote the closest collaboration between anthropologists, serologists and geneticists, the Institute arranged a one-day symposium, to survey work done, in progress and in plan, and to consider plans for the future. Several papers were read by specialists in the field of research. In the afternoon, there was a general discussion on organization (Miss Tildesley in the chair). Professor Fleure, who had presided at the opening of the meeting, asked that a reference centre should be set up to co-ordinate work being done, to tabulate and analyse results and data obtained and to be obtained, to act as a clearing house and information bureau, and to promote and assist fieldwork and publication; he proposed that the centre be set up under the aegis of the Institute and that a

Committee be nominated by the meeting (for appointment by the Institute's Council) to go into the matter, make plans and obtain financial support. The meeting welcomed the proposal and there was discussion of the scope, functions and staff of the centre. Dr. A. C. Allison (Oxford) spoke on blood-group fieldwork, the need to decentralize and to do as much analysis as possible on or near the spot. Professor R. A. Fisher welcomed the proposal and asked that when the committee was considering the organization of the centre it should include in its budget an allowance for travel as a regular and normal part of the salary of the person in charge of the centre. Dr. W. d'A. Maycock described methods of collection of data in a blood-transfusion centre. The size of the centre, its equipment and staffing and the question of training for research and fieldwork were discussed; suggestions were made regarding sources of income. The chairman, Miss Tildesley, offered to the proposed organization a home in the Institute's quarters in the new Science Centre; it was, however, realized that a temporary home must be found long before that; the urgency of the need was stressed, to save data which would otherwise be wasted. Among those who contributed to the discussion were Professor Fleure, Professor Fisher, Dr. A. C. Allison, Dr. Trevor, Dr. Roberts, Dr. G. Discombe, Dr. Lehmann, Mr. Christie, Mr. Luce, Dr. Fraser Roberts, Dr. W. d'A. Maycock, Dr. C. D. Darlington and Dr. A. E. Mourant. The meeting recommended to the Council of the Institute that a blood-group reference centre be organized by the Institute and that a committee be appointed to consider ways and means to that end—the committee (subject to ratification by the Council) to consist of:—Dr. A. C. Allison (Oxford); Dr. C. D. Darlington, F.R.S.; Professor R. A. Fisher, F.R.S. (Cambridge); Professor H. J. Fleure, F.R.S.; Dr. W. d'A. Maycock (Lister Institute of Preventive Medicine); Dr. A. E. Mourant (Blood Group Reference Laboratory, Lister Institute, London); Dr. J. A. Fraser Roberts (London School of Hygiene & Tropical Medicine); Miss M. L. Tildesley, M.B.E.; Dr. J. C. Trevor; Dr. I. M. Watkin (National Blood Transfusion Service, Cardiff); and the President, Honorary Secretary, Honorary Treasurer and Honorary Editor of the Institute (*ex officio*).

Summaries of the communications are given in the succeeding articles.



**Blood Groups and Mating Groups.** *Summary of a communication by C. D. Darlington, D.Sc., F.R.S.*

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The results of a first survey of blood-group frequencies in the world could be represented on maps as a diagram of contours or isogens. These in turn represented gradients or clines similar to those generally found for gene differences in any other polymorphic species of animal. In Europe, already for 15 years, the study has been carried far enough to demonstrate a correspondence with the great prehistoric movements of peoples. In doing so it has clinched the argument for the genetic control of linguistic development by racial groups. The small-scale contour map is however only a beginning. The high density of blood-group records in this country makes it possible to construct large-scale frequency maps. These will show the effects, not of movement, but of settlement.

Genetic principles tell us to expect, under settled conditions, the formation of mating groups with sharply distinct gene frequencies. Such mating groups are the basis of the genetic differentiation of tribes and races in all sexually propagating animals and plants. In order to test this expectation the detailed analysis of blood-group frequencies, village by village, has been undertaken in different parts of England, Scotland and Wales.

Nowhere else than in our country could a knowledge of the history of the people be used with such telling effect. And in no other species than man could such a decisive and instructive evolutionary experiment be set up. The results already obtained confirm our expectation of local or tribal differences in our own country.

*Summary of a communication by Professor R. A. Fisher, Sc.D., F.R.S.*

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From the early years of the century, initially with the OAB blood group of Landsteiner, it has been apparent that the serological testing of human beings would throw essential light, not only on the general development of human genetics, and on medico-legal problems of identity, parenthood and paternity, but as a new criterion in physical anthropology on the genetic relationship and migrational history of existing races. The promise of ethnographic significance offered by the OAB group has been emphatically confirmed by several other blood-group factors elucidated later. Ethnologists must be prepared for a development over the next twenty-five years of rather numerous sensitive racial discrimina, information about each of which will become only gradually available.

**Variations in the Frequencies of the ABO Blood-Group Genes in Northern England.** *Summary of a communication by J. A. Fraser Roberts, D.Sc., M.D., F.R.C.P.*

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A count has been made of 51,000 donors in the four northern counties of England and the North Riding of Yorkshire. The general frequencies over the whole area are much what might have been expected, but one of the main purposes of the survey was to discover whether underlying the broad English gradients there are in addition local variations. These have turned out to be very striking. There are big differences within the area, sometimes over quite small distances. It is possible to map the whole area empirically into a not too complicated series of sub-areas in such a way that all the significant heterogeneity lies between the sub-areas and none within them.

In the discussion of this paper Dr. G. Discombe said that it seemed possible that some of the results shown by Dr. Fraser Roberts had been injected into the material by his own methods of selection, thus producing the heterogeneity described. Further, when dealing with small villages, or other small groups, the existence of one individual of, say, Group A, indicated an increased probability of obtaining another Group A result on the next subject, and this source of variability needed further study.

Dr. Discombe's experience in dealing with small groups of volunteer donors had been that a high degree of selection occurs: donors of Group O, and Rhesus-negative donors, know that they are specially valuable and go about canvassing for donors of the same group. A more reliable source of data would be those from hospital antenatal clinics, which should be quite unselected.

**Blood Groups in Wales and the Marches.** *Summary of a communication by I. M. Watkin, M.Sc., M.B.*

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Wales displays significant variations in the frequencies of O, A and B genes. Very high O gene frequencies of c. 76 per cent. are confined to the north of the principality, whilst O frequencies of 70-75 per cent. are to be found as far south as the upper Severn valley. Only one or two remote mountain regions in South Wales harbour a population of very high O value—a fact which suggests that North Wales has not been subjected to the human migrations which have affected the southern half of the country.

O frequencies very similar to those observed in North Wales are also to be found in Ireland, in Scotland, among certain Berber tribes of North Africa, elsewhere along the Mediterranean shore and near the Black Sea coast. That the early inhabitants of western Britain had strong White North African affinities is suggested by the pre-Aryan element in Celtic syntax. A leading philologist has pointed out that 'the Celts show in the whole structure of their language a close affinity to the language of the white Mediterranean peoples of North Africa.'

A frequencies of an order observed in southern Scandinavia are to be found in a peninsula in Pembrokeshire and at the mouth of the River Dee, whilst there is some genetic evidence of Viking intrusions into Anglesea and near the mouths of the rivers Clwyd and Conway.

On the remoter moorlands where Fleure has postulated the existence of very early human stocks, B gene frequencies exceeding 10 per cent. are common and in the Black Mountain, Carmarthenshire, exceed 16 per cent.

Whereas it had formerly been supposed that populations with high B frequencies were among the latest to enter Europe and were confined to the east, it now seems as though a high B wave entered Wales not merely before the moderately high A current but possibly even before the very high O stream.

**Recent Advances in Serological Anthropology.** *Summary of a communication by A. E. Mourant, M.A., D.Phil., D.M.*

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Recent work, some of which is described by other speakers, has enabled new maps to be drawn showing the distribution of the ABO blood-group genes in Europe (these were demonstrated).

Surveys of the Rh blood groups have shown that the frequencies of these tend to vary less than those of the ABO blood groups within relatively small areas. The Rh frequencies are thus of particular value for distinguishing between regions of continental size. One area showing characteristic Rh frequencies comprises the whole of Northern and Central Europe, another includes all the countries surrounding the Mediterranean Sea and appears to continue westward to India. A third covers the whole of tropical and southern Africa. Eastern Asia, Australasia and America appear to form yet another division, but it may be that further research will subdivide this.

In Africa, investigations of the distribution of the ABO groups, of the Rh groups, and of the sickle-cell trait all converge to suggest that the Pygmies of the Belgian Congo are the modern representatives of an important component occurring in varying



amounts in most of the peoples who live south of the Sahara desert.

The American Indians and Eskimos show a high frequency of group *M*, and an almost complete absence of *B*. In South America, *A* also appears to be absent, as well as the newly discovered 'Duffy-positive' group. In most of these respects, American aborigines differ considerably from the population of eastern Asia and the Pacific Islands, though they resemble them broadly in their *Rh* groups. Thus blood-group studies have so far given little positive help in the search for the origin of the first Americans.

*A communication by Professor R. Kherumian, École d'Anthropologie, Paris*

**163** Des différentes communications que nous venons d'entendre, toutes pleines d'intérêt et de perspectives nouvelles, une conclusion s'est dégagée: la nécessité d'une étroite collaboration entre les sérologistes et les anthropologistes. Cette collaboration sera d'autant plus efficace que les notions fondamentales de leurs disciplines respectives seront mieux définies. Il s'agit en particulier de s'entendre sur l'emploi du mot 'race.' D'utilisation courante, ce mot recouvre généralement une notion confuse et mal définie. Cependant quelles que soient les divergences de vues entre les anthropologistes, il semble que sur un point l'accord se trouve réalisé. En général, on tient compte dans la définition des races de plusieurs caractères. Il en découle qu'il serait contraire à ce principe d'employer le terme 'race sérologique,' qui ne fait état que d'un seul locus. La notion de la race implique l'idée d'une communauté d'origine, proche ou lointaine. Or, les différences ou les similitudes d'un seul caractère (ou d'un seul locus) sont insuffisantes pour en juger. Ce n'est que par l'étude de l'ensemble des caractères génétiques qu'on peut espérer classer les populations selon leurs origines.

Il est à souhaiter par conséquent que les recherches sérologiques

soient conduites en étroite coopération avec la recherche anthropologique, seul moyen d'avoir une image exacte des parentés et des origines des diverses populations humaines.

*Summary of a communication by Dr. J. C. Trevor*

**164** The papers read at the symposium are a welcome sign of possible closer co-operation between 'academic' physical anthropologists and their serological colleagues. It is to be hoped, however, that the self-righteous tone so often adopted by certain transatlantic authorities on blood groups, in their rather summary dismissal of well tried methods of anthropological investigation, will not find an echo in this country. An impassioned blood-versus-bones controversy can help nobody, and may tend to reinforce the not altogether unfounded suspicion on the part of the 'academics' that serologists concerned with the classification of mankind have considerably more to learn than to unlearn from conventional physical anthropology. Although it is glossed over a little too lightly in the impressive writings of Professor William C. Boyd, the late Dr. Matthew Young's demonstration (in *MAN*, 1928, 116, 127) of some of the shortcomings of the ABO system, at least, has never in fact been controverted. This illustrated quite forcibly, almost a quarter of a century ago, the danger of reliance on a single criterion in human taxonomy. While it seems unlikely that the existence of, say, Neanderthal Man could have been inferred on serological grounds, the prospect of additional weapons in the anthropographer's armoury, furnished by systems other than the ABO, is sanguine in more than one sense, and the development of research into these should particularly be encouraged. Another line of attack that might yield results of interest is the determination of the blood groups of large samples of the bones of different races and of different ages. Several such series are available for study in Great Britain.

## SHORTER NOTES

**Tribal Sculpture: A Review of the Exhibition at the Imperial Institute.** By Henry Moore

**165** *Hon. Editor's Introductory Note*

Since the exhibition of Traditional Art from the Colonies in the Art Gallery of the Imperial Institute (until 30 September), to which reference has already been made in *MAN* (1951, 124), has on the one hand been selected by reference primarily to aesthetic standards, and on the other arranged so as to do as little violence as possible to ethnological as well as to artistic display methods, it provides, like the Royal Anthropological Institute's similar but smaller exhibition two years ago, an occasion for anthropologists to review that branch of cultural and social anthropology which is (or should be) most concerned with man's aesthetic impulses. When Haddon and Balfour, in the eighteen-nineties, were studying the evolution of decorative art, the concept of sculptural form had hardly begun to take the prominent place which it was soon to acquire in European art, for the reproduction of natural forms in stone, wood or bronze was still taken for granted; their researches were concerned almost entirely with the enhancement of surfaces rather than with the treatment of volumes. It is now generally conceded that the comparative study of the attitudes of tribal artists to their subjects and their materials is a proper part of ethnology; but extremely little work has actually been carried out in this field.

Unfortunately, the tribal artists are not available for questioning in connexion with this exhibition, and, if they were available,

would certainly not be articulate in terms of Western aesthetic theory. The Hon. Editor felt, however, that the views of an eminent sculptor—famous both for his profound appreciation of the essential qualities of primitive form and for that respect for the nature of materials which can perhaps be regarded also as a higher aspect of technological skill—might well bring out points worthy of field study by anthropologists. In generously agreeing to contribute to *MAN* his observations upon the exhibition, Mr. Moore made the condition that he should be confronted during his visit to the show with a series of questions designed to elicit answers which would have an anthropological bearing. The questions follow, with the answers which Mr. Moore has set down. This subject would seem to be relevant to discussion of whether anthropology is an art or a science or both, and the Hon. Editor will be glad if this review should lead to discussion of these problems in *MAN*. It need hardly be emphasized that the word 'primitive' is throughout used in a sense wholly complimentary to the master sculptors whose works adorn the exhibition.—Ed.

*Do you think that it is possible to distinguish among these exhibits the elements that are the contribution of original inspiration as opposed to the mechanical reproduction of traditional forms? How is one to tell whether to ascribe to a particular primitive sculpture the status of an original work of art or that of a competent copy comparable, say, to one of van Meegeren's copies of Vermeer?*

Looking at these sculptures, for example those from the Chamba,<sup>1</sup> Ashani<sup>2</sup> and Okuni<sup>3</sup> tribes of Northern Nigeria, I



think that even where a traditional form is followed fairly closely, there are many subtle touches which a poor artist would have missed. All these pieces have been selected for their quality, and practically every one of them has the 'flicker' of originality. Of course, it is difficult often to say whether the artist merely recognized these touches and copied them or whether he thought of them himself—but the Chamba figure<sup>4</sup> seems to me to have been conceived by an individual, and I should think that it has a special rhythm and set of proportions, and a unique expression in the head, for example, not to be found in any other Chamba figure.

*How far do you feel that these sculptures show domination of the artist by his material?*

I should say, on the contrary, that the material is dominated by the artist in almost every case. The soapstone figures from Sierra Leone<sup>5</sup> have a quality of stoniness about them because the artists have avoided the more deeply carved and slender forms which are easily possible in wood, but they show a mastery of the possibilities of stone; they are not just incised lumps of stone—but have forms fully realized in the round. In most of the wood carvings the sculptor has imagined something which has no relation to the original form of the tree trunk. In the Bali elephant mask from the Cameroons<sup>6</sup> the artist has imagined a shape—everything a child would remember about an elephant—which could not possibly have been suggested to him by the block of wood from which he started.

The imagination of the artist has been equally free in the Okuni antelope mask with long slender horns<sup>7</sup>, or in the complicated Akumaga mask from the Jukun<sup>8</sup>, or the great ram head from Owo<sup>9</sup> in which the neck and breastbone have been carved down to a deep, narrow, wedge-shaped form of astonishing power.

*Do you think that the static, four-square qualities of most of these works are to be regarded as restrictions inhibiting the artist's genius or rather as a useful discipline which assists its expression? In fact, is 'primitive' art to be regarded as limited, when compared to 'civilized' art?*

The only piece of primitive sculpture I can call to mind which represents actual physical movement is the Borneo dancing figure<sup>10</sup> from the British Museum, in this exhibition, which I well remember from my earliest visits to the Museum. But I do not think that representation of movement is at all necessary to the finest sculpture, or that its absence is a crippling restriction upon the artist; there is so much else left for him to do. (In fact, most of the finest carved sculpture of the past, of all periods and styles, is static.)

As to the 'four-square' posture of most primitive figures, I think that this is a limitation on the artist. But even then there is a tendency to avoid exact symmetry: the Chamba figure has a definite twist in it, and the life-size figure of a woman from Bèkom in the Cameroons<sup>11</sup> has its folded hands placed to the left of the centre line of the body.

*Is it possible from a study of the sculptures themselves to discern where the carver has transcended the religious or magical basis of the sculpture and produced what is, to all intents and purposes, art for art's sake?*

I feel certain that this never happens in primitive art. A man must have something more than an intellectual interest in art before he can produce a work of art. All of these works seem to me to be based on much more than just aesthetic impulses. I do not think in fact that any real or deeply moving art can be purely for art's sake.

*To what extent can you correlate high finish, smoothness of surface, with a particular kind of form? Or, to what extent is the high polish of some styles essential to their nature as works of art?*

Some pieces, such as the warthog mask from the Nafana of the

Gold Coast<sup>12</sup>, must certainly have been conceived with a highly polished surface, and would look unfinished without it. But all these masks and figures show some finishing on at least the salient surfaces—even the masks of the Kalabari Ijo<sup>13</sup>. Though the adze marks are still visible on many medium-sized pieces, they are usually softened by partial finishing. On big sculptures like the Urhobo village fetish<sup>14</sup> rough adze marks are left, but they are in scale with the carving as a whole.

High polish brings out the special quality of some materials—but finish and high polish in sculpture is often a matter of the size of the work. If the work is small and the sculptor wants to show subtle shapes or small refinements of detail he must have 'finish'—but on a large piece definition of shape can be got without it.

*Which sculpture would you select as showing the best use of the special qualities of wood?*

The Chamba figure seems to me one of the best from this point of view. The carver has managed to make it 'spatial' by the way in which he has made the arms free and yet enveloping the central form of the body. A rather similar effect can be seen in the large Owo ram head, with its great forward-curving horns framing the head and breastbone. But very many of the sculptures show a good use of the special qualities of their particular wood.

*How can we counteract the impact of European aesthetic concepts on the tribal artists? How would you set out to convince a school of tribal artists of the value of their traditional style, as compared with the European concepts of photographic realism which threaten it? Is there any hope for the preservation of these primitive values?*

I do not believe that it is any use to try to keep primitive art going or to shield the African artist from outside influences. All that we can do is to see that all the good tribal sculpture is preserved from destruction so that it can be put on show in wonderful exhibitions like this—here and in the colonies—to teach young artists what real vitality is. Nowadays artists have a greater opportunity than ever before of studying all the great traditions in the history of art, and working their way through them until they find their own style; at first, they may imitate Picasso, or primitive art, or the painters of the Renaissance, but eventually, if they have it in them, they will find their own way. So with the young African artists. What they have to learn from tribal art is not how to copy the traditional forms, but the confidence that comes from knowing that somewhere inside them there should be the vitality which enabled their fathers to produce these extraordinary and exciting forms.

#### Hon. Editor's Notes

<sup>1</sup> Catalogue (*Traditional Art from the Colonies*, H.M. Stationery Office, price 9d.) Nos. 149 (see Underwood, *Figures in Wood of West Africa*, 1947, Plate 27) and 150 (Underwood, *Masks of West Africa*, 1948, Plate 42).

<sup>2</sup> No. 151 (Underwood, *Figures*, Plate 26).

<sup>3</sup> Nos. 146 and 147, now in the collection of M. Charles Ratton, were formerly in the possession of Mr. W. O. Oldman, when they were described as from the Okuni, a tribe not otherwise traceable. No. 146 is shown as Plate 24 in the *Illustrated Handbook of the exhibition (Traditional Sculpture from the Colonies)*, H.M. Stationery Office, price 2s.), and also appears (No. 306) in *African Negro Art*, New York (Museum of Modern Art), 1935.

<sup>4</sup> No. 149; Underwood, *Figures*, Plate 27. This figure is, as Mr. Moore surmises, unique; with one exception (itself very different in conception) the other Chamba figures in the same collection in the British Museum are crude and lifeless, and the same is true of those published by Frobenius in *Das Unbekannte Afrika*, Munich, 1923, p. 145.



<sup>5</sup> Nos. 48-50; Handbook, Plate 30.

<sup>6</sup> No. 56; Handbook, Plate 25, or Catalogue of R.A.I. Exhibition, *Traditional Art of the British Colonies*, 1949, Plate 17.

<sup>7</sup> No. 147.

<sup>8</sup> No. 148; Handbook, Plate 23.

<sup>9</sup> No. 90a; (Handbook, Plate 15, and MAN, 1951, 124, Plate Fb.

<sup>10</sup> No. 3; Handbook, Plate 34, or Adam, *Primitive Art*, 2nd ed., 1949, Plate II.

<sup>11</sup> No. 58; Handbook, Plate 26, or Ernst Vatter, *Religiöse Plastik der Naturvölker*, Frankfurt-am-Main, 1926, fig. 19, left.

<sup>12</sup> No. 70; Underwood, *Masks*, Plate 12.

<sup>13</sup> Nos. 129-131; R.A.I. Catalogue, Plate 12.

<sup>14</sup> No. 126; Underwood, *Figures*, Plate 24.

## Second Pan-African Congress on Prehistory

**166** The Hon. Editor of MAN is informed by the General Secretary, Dr. L. S. B. Leakey, that arrangements have been made for the Second Pan-African Congress on Prehistory to be held in Algiers from 29 September to 4 October, 1952, with excursions during the preceding 14 days. The Organizing Secretary will be Professor L. Balout, Bardo Museum, 3 Rue Franklin D. Roosevelt, Algiers, to whom enquiries should be addressed.

It will be remembered that the Congress was invited, by

Field-Marshal Smuts, to hold its second session in South Africa this year, but the invitation was later withdrawn by the South African Government.

## Some Yugoslav Contributions to Physical Anthropology.

*A note by Professor H. J. Fleure, F.R.S.*

**167** The following publications by Professor B. Skerlj of Ljubljana, published there in 1950, have reached the Royal Anthropological Institute: 'Development of Secondary School Students of Ljubljana'; 'Anthropometrical Differences between Normal and Consumptive Youth'; and (with J. Kastelic) 'The Slav Necropolis at Bled.'

Professor Skerlj finds that the indications of the stages of puberty come earlier among girls of grammar schools (i.e. girls from well-to-do families) than among others. The differences may be about six months. Meat in the diet is conducive to early indications. He also finds a much greater incidence of tuberculosis among people of leptosomatic than among people of eurysoimatic or intermediate constitutional type. A group of early Slav skulls from Bled shows that the people were rheumatic and usually died before the age of 50; a few were of extreme long-headed type, but most were ordinary long heads. Unfortunately the dividing line is taken at 70; it would have more meaning at 72.5.

# REVIEWS

## AFRICA

**Cymbales et Crotales dans l'Égypte ancienne.** By H. Hickmann.

*Extrait des Annales du Service des Antiquités de l'Égypte, Vol. XLIX, Cairo, 1949. Pp. 95, text figs.*

**168** This study is a very remarkable piece of intensive research. Few more thorough monographs of this kind are known to me, and I welcome it as inaugurating a new era in the musical aspects of Egyptology. Dr. Hickmann has spared no pains to gather together every available fact regarding these ancient Egyptian musical instruments, and here presents us with a complete history of them from their earliest known beginnings to their present-day survivors in the land of their origin.

After dealing thoroughly with the necessarily restricted bibliography on the subject, Hickmann, in treating of the nature of the ancient Egyptian cymbals, describes in detail every known specimen, wherever situated, and illustrates them all by means of excellent photographs. Plate-shaped and cup-shaped cymbals (both of which remind us of India) are considered separately, and having thus cleared the ground, Hickmann reproduces for us, and analyses, the more important ancient illustrations of cymbal-playing, and supports these by a scholarly review of the many descriptions of the subject from early civilizations, though he draws attention to the vital point that no early Egyptian accounts exist. This lacuna is filled, however, by Hickmann's deductions which are based upon his knowledge of Coptic cymbals as used in Egypt at the present time.

Having dealt fully with the cymbals proper, Hickmann proceeds to do the same for the small cymbals fitted with handles, and this leads him naturally to a consideration, in great detail, of the *crotales*, those precursors of the modern 'castanets' which were characteristic of ancient Egyptian music. In tracing their history the author draws attention to the artificial hands made of wood, which formed a useful and very efficient substitute for the human hands themselves, and further indicates the place played by this variant in the evolution of clappers in general.

The illustrations in this section of the paper are very numerous, and show clearly the extent of Hickmann's studies. The whole paper is, in fact, as exhaustive as any scholar could make it, and students of the history of musical instruments will be deeply indebted to the author for writing it.

PERCIVAL R. KIRBY

**Miscellanea Musicologica.** By H. Hickmann. *Extrait des Annales du Service des Antiquités de l'Égypte, Vol. XLIX, Cairo, 1949.*

*Pp. 28, 3 plates, text figs.*

**169** The four studies contained in this volume form Nos. III, IV, V and VI of a series which will without doubt be continued and, we may hope, continued for long.

The first of the present group is called 'Observations on the survival of Egyptian hand signs in Coptic liturgical song,' but it contains much more than this title would suggest. There is a danger that a study of this nature, which is of paramount importance to the student of music who is curious to know about the origins of his art, may be overlooked by musical historians, few of whom are likely to think of consulting Egyptian State Annals for information concerning the first beginnings of musical notation, which is really what this paper leads up to. Dr. Hickmann, an ideal and rare combination of musicologist, archaeologist, ethnologist, historian and linguist, has here presented us with a chain of evidence regarding the possible origins of musical notation which makes one think furiously, even if one is not immediately convinced by it.

Having witnessed a performance of portions of the traditional Coptic liturgy executed by a famous singer of that church, Hickmann was struck by the fact that the performer accompanied the ecclesiastical song by hand signs, which were manifestly connected with both the rhythm and the melody of the music, and which served not only to indicate to groups of singers the shapes of the melodies to be sung and the time of those melodies, but also as a device for facilitating the training of the singers themselves. The student of musical history will, on reading the description of the performance, remember that similar devices were also in use in Europe in the Middle Ages in ecclesiastical circles. But Hickmann, in addition to describing the performance, has taken the trouble to photograph and reproduce the principal signs, and these he has classified and analysed, demonstrating that the suggestion made by Fleischer, in 1895, that the mediæval 'neumes' may have been derived from hand signs is apparently correct, and that the 'neumes' themselves represent the attempts of mediæval musicians to trace the melodic signs on parchment.

Now many writers have maintained that these hand signs originated in Egypt, though similar signs were in use in ancient



India in Vedic times, but hitherto the true significance of the signs and their connexion with musical notation through the art of writing has been almost completely overlooked. Hickmann, however, by his comparison of the Coptic signs with paintings and reliefs of ancient Egyptian musical performances, has not only been able to give to the latter an explanation which has hitherto eluded both musicians and Egyptologists, but has even gone so far as to suggest that certain of the hieroglyphics which involve the delineation of the hand and forearm might well be re-examined with a view to their possibly having a musical, as well as a literal, significance.

This daring suggestion may, if it proves correct, revolutionize our knowledge of ancient Egyptian music, for, although we know a good deal about the musical instruments of those early days, and even a little about methods of performance, of the actual music we do not possess a trace.

The second, third and fourth studies in this volume deal respectively with a shell whistle of the prehistoric period, a small shoulder harp and several precursors of the short and indented lutes. All three, though slighter in character than that already considered, display the same careful and systematic observation of the scientist, and again lay before us valuable data which might easily escape attention and which certainly merit preservation, particularly in the form in which they now appear.

Any new facts that may throw fresh light upon the musical practices of man's earliest days on this earth are of the greatest value, and Hickmann's suggestion that shells have been used in those days both as signal whistles and as dancing rattles is both novel and interesting.

In the two papers on the Egyptian lutes Hickmann has admirably applied what I may call the principle of stratification and, as a result, has given us a picture of the evolution of the various forms of musical instruments of this nature. It is particularly interesting to read Hickmann's evidence of the existence of precursors of what we of today regard as the true lute, the pear-shaped instrument which spread over Europe from the time of the Moors, and which is still made and played by the modern Arab.

PERCIVAL R. KIRBY

**The Religion of Ancient Egypt.** By Samuel A. B. Mercer. London (Luzac), 1949. Pp. 460, 118 text figures. Price 45s.

**170** The author claims for his treatise that it must inevitably be based upon myths and legends, poems, songs, hymns and prayers, narratives, biographies, histories and other literary remains, as well as upon what the classical writers have handed down, and he admits that a modern scholar 'with limited insight into ancient ways of religious thought' can only hypothetically reconstruct this ancient religion—'never one' but consisting of 'many cults at many places.' The book, thus, is not—nor does it pretend to be—the work of a student of comparative religion. The author—putting his trust in Manetho as a historian—assumes that 'Menes' is Egypt's first historic dynastic pharaoh who reigned some 3,000 years B.C., but he considers that the results of modern archaeological research are of greater value to the student of 'the facts of ancient Egyptian religious thought and practice' than anything to be learned from such writers as Herodotus, Eusebius, Diodorus Siculus, Plutarch, Apuleius and Plotinus. He takes Oriental symbolism somewhat literally: the Egyptians 'saw heaven as an immense and friendly cow' and 'imagined heaven as a woman, bending over the earth' (p. 21). His pages, however, are full of references to actual pictorial representations of this sort, duly figured, which should help the student to find his way about the extant remains of Egyptian symbolic art. He accepts what he calls 'legends' as the naïve explanations of how the cosmos came to be. The student of ancient religions, however, if he learns to compare the creation 'legends' of different historic civilizations, will realize that popular 'legends' or 'myths' of cosmogony, together with their *dramatis personae*, are mostly derived from a repertory of religious masques or pantomimes of the same kind as our European mediæval Christian mystery plays (themselves very often popularizations of adaptations by the early clergy of pagan 'mysteries'), and that these dramatic performances largely derived from traditional poetical allegories,

handed down orally and thus frequently modified to suit particular occasions and different local—indeed national—audiences. There exist texts, not only of mediæval European 'mysteries' (e.g. of 'Sainte Barbe,' 1557) but of Babylonian times (e.g. 'Epic of Creation,' Langdon, and 'Epic of Gilgamesh,' Campbell Thompson); fragments of such texts incorporated in so-called Japanese 'chronicles' (see my review of Kato and Hoshino's translation of the *Kogoshui*, A.D. 807, MAN, 1928, 20), and, of course, Egyptian, which illustrate my point.

The author does not help us very much to understand the religious thought of the ancient Egyptians, but he does assemble between the covers of a single book a gallery of the many stylized figures, describing, where possible, their ritual roles, and usefully indicates his sources. This should be particularly helpful to the non-specialist in identifying and dating the animal and masked human figures met with in Egyptian religious art. He devotes a chapter each to the better-known deities and reproduces examples of how they were represented, but he also gives many figurations and descriptions of minor deities and demons as the Egyptian represented them. This factual part of his book is likely to be of more service to students than the chapters on 'Theological Systems' and 'The Two Great Cults' (of Osiris and of Re). He states that 'the religion of Ancient Egypt was definitely polytheistic.' Like most European scholars (in this influenced by the Hebrew Scriptures) he takes the Oriental many-sidedness of the postulated Creative Powers—involving many representations and symbols—as proof of the concept of a plurality of anthropomorphic gods and goddesses. It is true that simple people in all ages and climes tend to accept stylized individual representations as convenient images of the unseen powers—the tendency against which the Semitic religious leaders have always contended, but which temple officials, jealous of their vested interests in the votive offerings of the devout, as well as the superstitious everywhere and in all ages, have encouraged. Probably the religious philosophy of the peoples of past times cannot be grasped without such clues as ecclesiastical art provides. It needs to be stressed, however, that such symbolism (as, e.g., the Catacomb representations and symbols of the Christian Messiah) is only a clue and that to understand these conceptions a key to the symbols and stylizations is not enough. Comparison with the cults and 'myths' of other historic civilizations that have left their mark in the folklore of those who came after, and a study of the writings of near contemporaries in neighbouring lands—even when their authors do not understand fully the meaning of what they reported—is more helpful than crediting the ancient makers of the protective and votive images with the literal minds and anthropomorphic bias of present-day Western peoples. More students of the calibre of the late Sir John Woodroffe and Madame Alexandra David-Neel are much needed in this field, for civilizations pivot as much on the ideas of their thinkers concerning the cosmos, past and present, and man's relation thereto, as on their economy. The student of ancient Egyptian religion lies in no danger of forgetting the place occupied by the Nile and the desert in moulding the psychology and ritual of the people: what he is apt to ignore is the part played in their philosophic outlook by the still older religions of West and Central Asia. The author's chapters on 'The Idea of God,' on 'Worship' and on 'Mysteries' are not really helpful; he denies to the Egyptian any system of ethics. On such aspects of the Egyptian people's attitude to religion the student is likely to learn more from the relevant chapters in Margaret Murray's recently published *The Splendour that was Egypt*.

An adequate index and sketch map are provided, and a list of authorities cited.

V. C. C. COLLUM

**Monuments funéraires préislamiques de l'Afrique du Nord.**

By M. Reygasse. Paris (Arts et Métiers Graphiques), 1950. Pp. x, 134, 175 illus.

**171** For well over a quarter of a century Maurice Reygasse has been engaged in fieldwork and excavation in Algeria, and the results of his researches have been preserved in the showcases, reserve collections and files of the Musée Bardo in Algiers, of which he is Director. Now, thanks to the encouragement and practical assistance of M. le Ministre Naegelen, Governor General of Algeria,



we are to have published summaries of M. Reygasse's work. In this volume Reygasse deals with sepulchral monuments; his written notes are accompanied by a lavish collection of photographs, plans and maps: later volumes will deal with rock paintings and engravings, and with stone tools. If all the volumes are of the same standard of scholarship, lucid exposition and technical production as is the present memoir, they will be a fine memorial to M. Reygasse's great work and to the enlightened interest of the Algerian Government.

Reygasse distinguishes the following as the most characteristic types of pre-Islamic graves in North Africa: tumuli (simple heaps of stone), *chouchets* (cylindrical monuments with faced walls), stone circles, dolmens and rock-cut tombs. The tumuli and *chouchets* are Berber tombs and are found in all the areas colonized by the Berbers. One of the most interesting parts of this book is the account of the excavation in 1933 of the site of Tin Hinan in the Hoggar and the discovery of what may be the tomb of Tin Hinan herself—legendary ancestress of the Tuareg.

The dolmens of North Africa are confined to the extreme north of Algeria; except for the Aures district they do not occur in the Sahara. They occur in enormous cemeteries; that of Roknia dug by Bourguignat contained 3,000 dolmens. All these megalithic monuments are free-standing and Reygasse notes that there is no evidence to suggest that any of them were originally covered by a tumulus. He reviews all the arguments for their date, and concludes, as Gsell did before him, that they were built between the third century B.C. and the third century A.D. He emphasizes, again as Gsell did, the chronological gap between the *floruit* period of the West European collective tombs (2000 to 1400 B.C.) and the Algerian dolmens, and the difference between the two groups in constructional detail and in gravegoods. He thinks, and surely he must be right, that the megaliths of India, Madagascar, Algeria and Western Europe are independently developed, and that the resemblances between them can be explained 'par le seul déterminisme industriel.' A most valuable point which I commend to all those who see in megalithic architecture a world-wide link which they do not invoke to explain other widespread architectural similarities such as dry-walling. On this point alone, M. Reygasse's survey is worth the most careful consideration by all prehistorians.

GLYN E. DANIEL

**African Systems of Kinship and Marriage.** Edited by A. R. Radcliffe-Brown and Daryll Forde. O.U.P. for Int. Afr. Inst., 1950. Pp. viii, 399. Price £1 15s.

**172** This symposium of nearly a quarter of a million words consists of nine long essays by as many fieldworkers, preceded by an introductory essay on kinship theory by Professor Radcliffe-Brown. The essays examine the kinship systems of the Swazi (Kuper), Nyakyusa (M. Wilson), Tswana (Schapera), Lozi and Zulu (Gluckman), several Central Bantu societies (Richards), Ashanti (Fortes), Yakö (Forde), Nuba (Nadel) and Nuer (Evans-Pritchard).

The editors have, in their short preface, invited comparison with *African Political Systems*, published in 1940. For the most part, the same persons have contributed to the two books. Radcliffe-Brown's Introduction to the present volume fulfils much the same purposes as did his Preface to *Political Systems*: it 'offers an introduction to the general comparative and theoretical study of kinship organization.' It is a recapitulation fitting into precise perspective those important contributions to kinship theory and analysis which he has made over the past 30 years, much as his Preface of 10 years ago summarized his researches into political problems. In an enviable prose style, he has brought together the mother's brother, the equivalence of siblings, the joking relationship, rights in personam and in rem, as well as many other concepts which today are the social anthropologist's basic working tools.

What is lacking in the present volume is an introduction such as Fortes and Evans-Pritchard provided for *African Political Systems*. Each essay in the new collection contains valuable information. Schapera's quantitative study of consanguineous marriages among the Tswana tribes is exciting reading; Evans-Pritchard's exposition of the kinship bonds of all persons living in five Nuer communities is most valuable documentation for all his Nuer publications; Kuper's careful exposition of Swazi kinship behaviour (unfortu-

nately containing two errors in the charts) might well become a model for anthropological writing in this genre. Individual excellences could be cited from each article. But nowhere are we given a unifying theme, or indeed any assistance whatever in sorting the welter of problems investigated or the conclusions to be drawn from them. No overall standard of relevance is established, no hypotheses are suggested against which to adjudge and order the data.

Radcliffe-Brown has, indeed, in his Introduction, postulated two basic types in African kinship systems: the predominantly unilineal system (including double unilateral) and the cognatic system. This is not a sufficiently striking or controversial proposition, however, to set a tone for the book or to bind the essays into a coherent whole. That most of the individual essays do in fact have widely varying central themes only emphasizes the lack of a central axis along which the book can proceed. To list only a few of the more obvious of these themes: (1) the relationship between the kinship system and community grouping (Wilson, Forde, Evans-Pritchard); (2) the sociological content of the affective relationship between persons standing in different categories of kinship (Kuper, Fortes); (3) the interaction and eventual compromise between conflicting ideas of descent-reckoning (Forde, Nadel, Richards, Fortes); (4) social sanctions for maintaining kinship relationships (Wilson, Schapera, Kuper, Gluckman). The list could be trebled.

This diversity in basic approach among the contributors leads to an important question: Do kinship 'systems' lend themselves to comparison at the same level as political systems? What are the features—the data or abstractions—to be compared? Surely any one of these selected themes could be used as a basis for comparison. Actually, four of the nine essays in this book use a comparative method (Gluckman, Richards, Schapera, Nadel). Their results, though in no way conflicting, are themselves not comparable (with the possible exception of the first two), obviously, because they deal with different problems, although all are concerned with 'kinship systems' in one way or another. Though much the same may be said of 'political systems', they would seem—from the evidence presented in the two symposia—to be more tractable. A typology, even a rough, rule-of-thumb working typology, of kinship systems is—as Radcliffe-Brown recognizes (pp. 1ff.)—a most intricate and complex procedure, and one at all times relative to the problem in hand.

This review has cavilled at great riches; that is the feeling which the book arouses. However, the more material presented, the more is the pity that it was not integrated, with reference at least to some of the points raised by the contributors. As it stands, it is a source book which the Africanist cannot afford to be without. It might have been the mid-century milestone in kinship studies.

PAUL BOHANNAN

**The Systems of Land Tenure in the Kikuyu Land Unit: Part I, History of the Tribal Occupation of the Land.** By H. E. Lambert. Univ. of Cape Town, Communications from the School of African Studies, N.S., 22 February, 1950. Mimeographed. Pp. 185, 3 maps. Price 10s.

**173** This book is a good example of the kind of historical work that is needed as part of the foundations of a satisfactory history of the peoples of Africa. It demonstrates the need for the writer of this kind of history to have some knowledge of the people and their country, and shows how documentation is possible in an area for which, at first sight, no documents appear to exist. All records of the past actions of African peoples must depend ultimately on their own statements unless there exist records made by foreign eyewitnesses. Sometimes the latter can be found. But African statements, the people's traditional history, when methodically examined and checked by whatever supplementary material is available, must in fact remain the basis of their historical study for the pre-European period, and often for much of the European period as well. When we reach this stage, documents as a rule become available, even if they consist largely of travellers' accounts, reports of commissions, and the like; and Mr. Lambert shows how, provided the investigator has in addition to his other equipment the essential qualification of local knowledge, the value of travellers' observations and the statements made to commissions can be properly assessed. For the history



of the Kikuyu, the documentary evidence used by Mr. Lambert may be summarized as follows: (i) writings of European travellers up to 1900, including Krapf (1860), Thomson (1883), Teleki and von Hölnel (1887), and Neumann (1897); (ii) articles and memoranda by administrative officers and others; (iii) district political record books and handing-over reports; (iv) evidence given to commissions, e.g. the 1929 Kikuyu Land Committee and the 1932 Kenya Land Commission; and (v) studies by anthropologists, which are important because often it is only the trained anthropologist who can adequately interpret much of the evidence.

The Kikuyu Native Land Unit stretches for about 180 miles north-east from the neighbourhood of Nairobi across the lower south-eastern slopes of Mount Kenya. This Land Unit (a term which has officially replaced the more familiar 'Reserve') is the home of two major tribal groups: the Kikuyu in the south, and the Meru in the north, with a number of smaller tribes, the Embu, Chuka, and Tharaka, in between, the total population amounting to a little over a million. In the Kikuyu country there are also a few remnants (given by Lambert as 159 men, women, and children) of the Nandi-speaking Dorobo hunters who formerly 'owned' the land.

The book is divided into eight chapters. The first three give a summary of the tribal groups in the Land Unit, and their traditional history (pp. 1-43). The author's conclusions are that while the Chuka have no tradition of movement into their present country, evidence for the Meru and Tharaka points to their having come from somewhere to the east of their present habitat, and the same may be true of the Kikuyu. The main migration of the Bantu tribes of the Land Unit was, he thinks, part of a general movement of a pre-Nyika, pre-Kamba tribal group which began to come from near the coast north of the Tana at least five centuries ago, and continued till about the end of the eighteenth century.

Chapter IV deals with the earlier occupiers of the Land Unit (pp. 44-70). It gives a useful summary of what is known of the pre-Kikuyu Gumba and Athi, sometimes regarded as mythical, but clearly real people in spite of the legends which have grown up about them. More than half the chapter is devoted to the Dorobo (pp. 54-70), whose importance is that they were the people actually in possession of the land when the Kikuyu first appeared on the scene, and the history of the acquisition of the land by the Kikuyu is in fact the history of the relations between the Kikuyu and Dorobo. Although there is some useful and interesting information about the Dorobo here, especially in Mr. Lambert's account of the organization of the Keriita Dorobo, I find myself unable to agree with all

his statements and conclusions about them. But this does not affect his main thesis, the relations between Dorobo and Kikuyu; and he recognizes quite clearly that their social organization is linked with the spirits of the dead. The similarity of the beliefs of Kikuyu and Dorobo with regard to the spirits has considerable bearing on the land transactions between the two peoples, for, as Dr. Leakey says (quoted by Lambert, p. 94), 'the Kikuyu believed absolutely that if they took the land from the Wanderobo by force, the spirits of the Wanderobo who were killed defending their land would render the Kikuyu occupation of the land useless, because they would bring supernatural punishments upon them'; and, in Mr. Lambert's words (p. 125), the cult of the spirits 'may be taken to support the view that in accepting payment for the land, the "sellers" [i.e. the Dorobo] did not intend to alienate all interests therein completely and for ever from their groups.'

Chapter V treats of the acquisition of land in the districts other than Kiambu, and describes how it was acquired: the Meru just walked in and took it; the Embu and Mwimbi settled at the expense of the Chuka; while the Tharaka drove out or absorbed their predecessors the Njuwe (pp. 71-84). In chapter VI Mr. Lambert quotes extracts from the documentary evidence about the acquisition of land from the Dorobo in the Kiambu district (pp. 85-99) by means of sale and other methods. He examines the difficulties in the way of assessing native evidence, and in particular of defining exactly the significance of words like 'buy' and 'sell' used by European investigators, giving an illuminating statement about the words *gura* and *endia*. He also has some remarks on the political aspect of evidence on subjects like land under the heading 'Political obstruction to investigation.'

Chapters VII and VIII contain comments on the opinions recorded in chapter VI, with an analysis of the manner in which land was sold and the question of redeemability, and an investigation into the relationship between the land and the spirits, arising out of the reasons given by the Kikuyu why they did not simply take the land (pp. 100-128). These two chapters are perhaps of more interest to the anthropologist than the historian, though the latter will gain from them some insight into African thought. The last chapter contains an examination of the classification and social status of property, with special regard to land sale and transference of ownership.

This book will be of value to both historian and anthropologist. There is a full index, and misprints seem rare, though on p. 61, line 52, 'interior' should doubtless be 'inferior.'

G. W. B. HUNTINGFORD

## AMERICA

**Yalag: Una Villa Zapoteca Serrana.** By J. de la Fuente. Mexico City (Museo Nacional de Antropología), 1949. Pp. 381, 16 photographs, 2 maps

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This is a study of a mountain village in the Zapotec region and gives a useful account of the present conditions, written from the now prevalent viewpoint of interest in culture change rather than in survivals. The situation, climate, arts of life, agriculture, division of labour, economy, family organization, cycle of life, local government, religion and treatment of illness are all described.

The people speak Zapotec but are anxious for their children to learn Spanish and therefore prefer schoolteachers who do not know the *idioma* (Zapotec), so that the instruction may be all in Spanish. Naturally they think that those who speak and read Spanish will be better able to deal with legal and official difficulties, and this is their principal reason for desiring education.

As compared with the more conservative cultures surviving in Guatemala, one sees the advance of hispanicization. The author classifies the religious beliefs as either 'Catholic' or 'gentil' (heathen). The *brujos* practices are kept very secret and there is a tendency for their cults to disintegrate and disappear. It is noteworthy that there seems to be a native movement to suppress them. Many of their practices are said to have disappeared since the beginning of the century or to have survived only among other Zapotec groups or among the Mixe. On the other hand there is a curious desire to have

their own special form of Christianity. Those Roman Catholic clergy who insist on strict Catholicism sometimes get into trouble with their Zapotec flock and there have been instances of attempts to remove them. Probably, however, there is a considerable amount of native belief and practice still surviving, though the author was naturally not made aware of it.

A reader of this book would not know that such a thing as the ancient native calendar of Middle America ever existed, for it is nowhere mentioned. Yet there are hints of its former existence. In particular in the section on heathen ceremonies it is said that Candelaria, a February, or the days before and after this day, were known by a term translated in Spanish as *la boca noche primera*. Its approach excited terror among the people, who for about five days before that day appeared quite grave and preoccupied. Everyone was shut up in his house at an early hour. Within the house everything which was usually left open was guarded. The young and especially the children were prohibited from being out late, as they would be running great risks lest the boys might be missing from their houses. All this looks like a confused remembrance of the five unlucky intercalary days of the old calendar. Burgoa says the ancient Zapotec year began on 12 March. This was probably Julian, and if so it would in the sixteenth century be equivalent to 22 March Gregorian; that is, nearly the vernal equinox on 21 March. Again, the ritual of sowing, though now determined by dates in the



Christian calendar, reminds one of the sowing on lucky days of the ancient calendar in Guatemala. De la Fuente says that, before sowing, the prayermaker recites a rosary while his (female) associate answers the responses. On the day of sowing the same or another prayermaker is called on who says a suitable rosary then or shortly before. The sowing is on the day of an important saint or on a 'good' day agriculturally and in other senses, and often on the day of the cross (3 May), a good day *par excellence*. If the rains hold back after May it is believed it will rain on Saint Anthony's day. The fact that it frequently rains when this feast approaches, if the year is not one of absolute drought, may have contributed to the emergence of this saint. If this fails they sow on the day of Saint Peter, 12 June, using quick-growing maize. Other good dates are indicated by the sorcerers. It is thought right to be kind to dogs—especially black ones, as they help the souls over Jordan. The Earth acts and affects men at certain periods (not determined) which last thirteen 'times' from a date known to the sorcerers and called by them 'the day in which the world speaks'. The moon is the heavenly body which has most influence. At full moon they cut timber and make sowings and petitions to the souls and saints. Lunar eclipses are a sign of fire in the moon and announce misfortunes.

RICHARD C. E. LONG

**The Gallinazo Group, Viru Valley, Peru.** By Wendell C. Bennett. Yale Univ. Publ. in Anthropol., No. 43. Yale Univ. Press (London: Cumberlege), 1950. Pp. 124, 12 plates. Price 12s. 6d.

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This work is the contribution of Yale, in the person of Dr. Bennett, to the Viru Valley Program, in which a number of organizations studied the growth of culture in the valley from the earliest times to the present. The work was done in 1946, and while each organization worked independently on a particular aspect, there was close cooperation between them, and the Viking Fund provided apparatus and materials which were available to all. Thus Dr. Bennett concentrated on a particular group of ruins of one period, but was able to place them in their proper time sequence as the result of stratigraphical work by the Columbia and Chicago representatives, and he fitted each individual excavation into its place by comparison with a master chart of the whole pottery sequence prepared by a member of the staff of the American Museum of Natural History.

Dr. Bennett had worked previously on the Gallinazo group of ruins and had demonstrated the existence of a distinct culture there, but he had not been able to place it stratigraphically, though he suggested a post-Mochica date. It is now clear that this Gallinazo Culture, which has been named Negative or Viru by others, lies in this valley above the Salinar and below the Late Mochica. It is suggested that it may be contemporary with the Early Mochica elsewhere. The Gallinazo Culture, which is provisionally dated A.D. 300 to 700 is particularly characteristic of the Viru Valley, where it is widespread, but it is not altogether confined to it. The use of negative painting is an important feature on the grave pottery, but is not common on the utilitarian wares. It is clear that the funerary pottery was specially made for the purpose.

There was a general continuity in development throughout the period, without any change in type of population, but it is possible to divide it into three stages, chiefly on the basis of changes in building materials. Stage I is marked by the use of *tapia*, namely large blocks of puddled adobe. This is usually a much later feature, barely pre-Inca, and its use at this early date seems to be a peculiarity of this valley. Buildings at this stage take the form of small clusters of rooms without windows or doors, which must have been entered from above, implying a subterranean ancestry, but there seem also to have been open courts with walls ornamented with cut-out patterns or adobe mosaic. Stage II has a variety of materials, but the commonest are ball adobes. Larger groups of rooms, expressively described as honeycomb agglomerations, are found. These persist into the next stage, when doors are found for the first time. In Stage III, adobes made in cane moulds are used first, then plain adobes. These are both Mochica types.

A large proportion of the structures is made up of successions of

houses built on the ruins of their predecessors, five to eight levels being distinguishable, like a Middle Eastern *tell*, but true pyramids are found only in Stage III. There are certain mounds without houses, which have burials round their margins, but the main cemeteries have not been found. An estimate based on the number of houses in the group suggests a population of about 5,000 at a given time.

By comparison with others, for example the Mochica, the culture is rather drab, and there is no evidence of marked differentiation in class or occupation. It is suggested that the energies of the population found their outlet in subsistence activities, chiefly agriculture based on irrigation on a large scale. In Stage III, influences from Mochica and Recuay tend to modify this pattern. This is seen both in the introduction of pyramids and Mochica types of adobe, and in ceramics. Finally the Gallinazo Culture disappears under the impact of a Mochica invasion.

The name of our Honorary Fellow Dr. Bennett is a sufficient guarantee of the quality of this work. The evidence is lucidly presented and the comparisons and conclusions in the last two chapters carry conviction. It is not quite clear why it is necessary to coin a new name for a known culture when it is found outside its type valley, and it puts an extra strain on the reader to have to remember that Puerto Moorin means Salinar and Huancaco means Mochica, but this must be the responsibility of the Program as a whole and not only of Dr. Bennett. Some of the results of the Viru Valley Program have already been published, but a good many are yet to come, including the full publication of Junius Bird's epoch-making pre-ceramic discoveries. The contribution considered here stimulates the hope that all will soon be completed.

G. H. S. BUSHNELL

**Florentine Codex: General History of the Things of New Spain, Book I: The Gods.** By Fray Bernardino de Sahagún,

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trans. from the Aztec with notes by Arthur J. O. Anderson and Charles E. Dibble. Monographs of the School of American Research, No. 14, Part 2. Santa Fé, New Mexico, 1950. Pp. 46, 9 plates. Price 84

This is the first part to be issued of a work which when complete will comprise an introduction and the twelve books of Sahagún's work as they appear in the Codex Florentinus. All students of Aztec culture, apart from the fortunate few who read Nahuatl with ease, have felt the need of a careful translation of this Codex, which is the best surviving text. It covers the same ground as the Spanish text published by Kingsborough, and includes much material, particularly the Appendices, which does not appear in the version of the Madrid Codex given by Selser.

The authors are to be congratulated on preserving the original spelling of the Nahuatl text without making any attempt to modernize it, thus avoiding any doubts which might arise as to the exact meaning of a phrase. One is on less sure ground with the English text. They were instructed to employ a degree of archaism in phraseology conforming with that of Sahagún's Spanish. The result has been a very terse and forceful style which I find thoroughly enjoyable, apart from the word 'crazed,' which has a rather too modern connotation. The archaisms are fully justified in the Appendix where Sahagún is describing the Christian viewpoint, and is made to speak the language of the pulpit of his period in a most vigorous and lively style.

It is a pity that the illustrations are a copy of a copy, rather than direct reproductions from the original; perhaps this may be remedied in later parts. Unhappily, too, production difficulties have made it impossible to place the illustrations in the position in the text which they occupy in the Codex. They are all properly annotated however, and little is lost, apart from artistic unity.

The footnotes are excellent, throwing much light on this part of the work by cross references to the same subject in other parts, and giving Sahagún's Spanish text wherever there is a possible difference of meaning.

In fact this is an indispensable work of reference to all who would make a serious study of the culture of the Aztecs. Sahagún's scientific method of approach is too well known to be discussed here, but it



is useful to point out that his Indian informants, old men when he wrote his notes, were middle-aged at the time of the Conquest, so that the Books about Festivals and daily life are from fully informed sources; but that this first Book, on the Gods, is from information given by men who had not reached the highest ranks of the Priesthood. Hence arise some variations from the pre-Cortesian Codices which have caused confusion in the past. It is amusing to find that the supposed deity Tloque Nahuaque is here used as an epithet for Tezcatlipoca as 'Protector of all.' C. A. BURLAND

**Native Arts of the Pacific Northwest from the Rasmussen Collection of the Portland Art Museum.** *Introductory text by Robert Tyler Davis. Stanford Univ. Press (London: Cumberlege), 1950. Pp. 165, 194 illus. Price £2 10s.*

When the Stanford University Press decided to start the publication of its Art Series with a volume on *The Native Arts of the Pacific Northwest*, they probably did not realize what a great contribution they would be making towards latter-day artistic development. Artistic change of recent years has taken a wide swing in the direction of the primitive; Picasso and his modern school have influenced many and much development has, unfortunately, only been eccentric in form and design, in colour and shape, showing little real purpose. The book's illustrations show, in contrast to a modern trend, that the primitive art of these natives was both vivid and sincere: the peoples of the Haida, Kwakiutl, Tsimshian and Tlingit Tribes have been producing for a couple of centuries three-dimensional works of art which, though naïve, are graphic and tell a story, serving a purpose and exciting interest, though the highly decorative patterns may not always please. The striking illustrations in this book can hardly fail to stimulate fresh thought amongst interested students. It may even make them feel dissatisfied with so-called civilized eccentricities such as are now being presented to the cultured world.

British Columbian natives are an admixture of Mongols, who crossed from Asia via the Aleutian Islands, and North American Indians who were already in the area. The admixture has produced a depth and subtlety and a robust type of mind capable of producing works of art that justify careful investigation. Our Captain Cook first encountered these natives when cruising in that area in 1778. His appreciation of their work was far in advance of the time; in fact, it was over a hundred years before anything systematic was done to study their mode of life and to collect specimens of their work. The totem poles were the first to attract attention, during the early nineteenth hundreds, and many of them were removed. About the same time, a Mr. Axel Rasmussen was appointed to Skagway as a schoolmaster. He liked the people and made friends. He studied them and began seriously to collect their work, purchasing objects from them or receiving them as gifts; many important articles were lent to him by their Indian owners, and he allowed them to re-borrow them for special celebrations.

Rasmussen intended housing his fine collection in a museum in Skagway, but unfortunately he died before completing the project, and the collection was divided. Happily, however, Mr. Earl Stendhal of Los Angeles knew of its existence, gathered as much as possible together again, and brought it south, where it was finally housed in two galleries of the Portland Art Museum. This material forms the basis of the delightful book under review, which not only records the native arts of the Pacific Northwest, but also indicates the culture of the people, their skill, capabilities and craftsmanship. Indeed, I question whether such ability and invention has been surpassed by any other native race within the British Empire.

They worked with very primitive tools. Early woodcarvings were cut with stone adzes, later examples with iron. It was no mean feat to carve a great totem pole, 40 to 50 feet high, with such implements. Not only did they carve wood, they carved slate as did no other primitive people. Horn, also, they shaped and carved, softening it with steam and then moulding it into spoons, bowls, drinking cups and quite delicate ornaments. They fashioned square wooden boxes out of straight planks, by moulding and bending and grooving, without actually cutting through the timber; these they

then decorated with abalone shells and frequently painted in bright colours. They took wool and worked it with a tapestry weave into their famous Chilkat blankets. The colours used were striking blacks, yellows, blue-greens, reds. Mountain goats provided them with a fine wool for ceremonial or festive garments. Headdresses were frequently decorated with human hair, ermine skins, the down of birds, or seal skin and walrus bristles. They carved ivory from walrus tusks as did the Orientals. Copper they took and made into forms of currency, flattening it out into keystone shapes, often three feet in height, which they used in exchange for either six wives or eighteen cows.

They were a peaceful people. Aggression and fighting was not their mode of life. They appreciated music and made a whole range of instruments. Their legends are not warlike, but deal mostly with relationships of men and animals, and the designs created by men were harmless and usually of birds, beasts and fishes, while the women based their designs on more inanimate objects.

They liked colours, and used mineral dyes from local materials, but when they came in contact later on with white men, they bought their colours. They used buttons in great profusion and in elaborate patterns. Their earlier work was of a slightly higher standard; later, when making for trading or selling, this was sometimes lowered. For themselves they created objects for special events, such as totem poles for a potlatch. Unlike other primitive people of North America they lived in wooden houses, not in cliff dwellings or wigwams. One house of considerable size harboured several families. Houses were coloured and decorated with carved corner or house posts. Roofs were peaked, walls were without windows. The entrance was a circular hole, so contrived that while people could get in, animals marauding at night were kept out. Families divided themselves around the walls of these houses, where they had deep shelves for sleeping and keeping their possessions. Floors were of pounded earth with a fire in the centre, and ceremonial occasions in these houses must have been impressive, with the remarkable cloaks, blankets, head-dresses and carved wands of the participants emerging, as they did, from the gloom of corners into the bright flickering firelight.

In his interesting introduction, Mr. R. T. Davis deals with sidelines of their life. For instance, there existed laws of inheritance seldom found elsewhere. Legends, songs and poems belonged exclusively to a family and though others liked to hear them, they could not use them.

The most northerly Indian tribe were the Tlingits, but even farther north lived the Eskimo, whose work is also illustrated in this volume. It had to be created in spite of a dearth of material. They had no forests, so driftwood was assiduously dried and collected for their craftsmen. The shortage of this material meant that they had to design objects to give a sense of size and substance which did not actually exist. As their climate is more extreme and severe, their work was simpler and cruder than that of the southern tribes, though the Eskimo learned much from them.

The area occupied by the Indians is bounded on the west and east by the Pacific and Rockies, and on the south and north by the Fraser River and the Eskimo. The climate of this area is mild and the men seldom used to wear clothes except on ceremonial occasions, though the women did. They are a clean and hardy people, and used to break the ice in winter to bathe. They roam freely, but always stick to their families and clans, and maintain their customs and habits.

This book is to be highly recommended: not only does it thoroughly illustrate an art no longer practised; its analysis brings to designers a message fresh with grace and charged with intuitive skill—an art that is vivid and ingenious, always considering the materials it has to employ. I feel that it can be studied with great benefit.

There are only three other comprehensive collections of the work of these people outside Canada: the Rice Collection in Chicago; that in the New York Museum, and my own here in England. The four collections are about the same in size and cover the same artistic groupings. This book will undoubtedly now focus attention on the work of these interesting people as nothing else has done before.

A. C. BOSSOM



**Paiute Sorcery.** By Beatrice Blyth Whiting. Viking Fund Publ. in *Anthrop.*, No. 15. New York, 1950. Pp. 107, map, tables. Price \$1.50

**178** Dr. Whiting's monograph is a study which covers far more than its title implies. Following a brief account of Harney Valley (Oregon) Paiute economy and history, the author deals very adequately with personal supernatural power, disease, therapy, sorcery and the control of aggression in the family and band. Her main theme is the role of sorcery in social control, and to this end she classifies mechanisms of social control into conscience, public opinion, reciprocity, supernatural sanctions, retaliation and agencies to whom authority is delegated. These categories are not of the same kind: supernatural sanctions are carried out by conscience and public opinion. Also reciprocity, even when reinforced with a threat of breaking the relationship when obligations are not fulfilled, is hardly a control mechanism; the sanction in this case is unfavourable public opinion or personal retaliation. Sorcery is treated within this typology: fear of sorcery and the accusation of sorcery were major control mechanisms which operated through conscience, public opinion, supernatural sanctions and threat of retaliation by physical means.

The ethnographic account itself is excellent. Dr. Whiting shows, for example, that a Paiute doctor's diagnosis of illness as caused by sorcery occurs when the sick person is a party to a quarrel or related to a disputant, while other cases of illness are diagnosed as caused by an encounter with a ghost or, if the patient is young, by his own power. She describes how a person's reputation for sorcery begins with the person's 'mean' temperament and with his having relatives who are also 'mean,' and how it develops through community gossip, circumstantial evidence such as threats and quarrels with a victim or his relatives, and the official accusation of a doctor. When such a reputation is acquired, any illness befalling a person in any way connected with someone with whom the 'sorcerer' has quarrelled is attributed to the 'sorcerer.' Furthermore, deaths which occurred years before the reputation was gained may be later attributed to the 'sorcerer.'

The author states that within the family aggression is inhibited mainly by emotional ties and the need for co-operation; however, in the band it is the recognized practices of retaliation, sorcery and accusation of sorcery which serve to restrain aggression against others and provide the penalties for anti-social behaviour. This is an interesting and perhaps generally useful distinction, but the sense of 'family' is vague. The aboriginal household had for its nucleus an elementary family and usually included siblings of the male head; it often included also the spouse and children of his daughter or son. Camp groups, which co-operated in some activities, were composed of several households linked by close kinship or affinity. Marital, household and camp ties were easily broken. It is thus difficult to judge the intensity and extent of family loyalties. The present settlement is larger than the camp, but not all close relatives are in the settlement.

The book concludes with a cross-cultural study in which the co-existence of sorcery and 'co-ordinate control' (absence of super-ordinate social control) is tested in 50 societies. Sorcery as an important control mechanism is found significantly correlated with the absence of formal judicial or punishing agencies.

PAULA BROWN

**Fighting with Property.** By Helen Codere. Monographs of the Amer. Ethnol. Soc., No. XVIII. New York (Augustin), 1950. Pp. viii, 136. Price \$3

**179** This short study is a revaluation of the extensive literature on the Kwakiutl of British Columbia from the late eighteenth century to the present time. The author, taking the potlatch as her *leitmotiv*, has traced acculturation of this people during some hundred and fifty years. Her conclusions, briefly, are that Kwakiutl population decreased steadily during the contact period until 1924, with consequent realignments (not adequately documented) in the system of status which found expression in potlatch and warfare. White administration, coupled with tendencies within the society, put a stop to war. The rivalry which had heretofore been expressed in both potlatching and war came to be centred wholly in the potlatch, thus providing impetus for its continuation and extension. A second, contributing, impetus was supplied by the influx of cheap manufactured goods—particularly woollen blankets—and currency, which made it possible for much more impressive amounts of goods to be drawn into the potlatch system.

Though all in all Dr. Codere presents a coherent and convincing picture, a few of her arguments can be questioned. Some of her data extend back beyond the historical period—she cites one instance in which family histories contain potlatching activities covering 23 generations. The author assumes that since these records show the amounts of goods in the early, pre-historical, potlatches to be smaller than those of the earliest historical accounts (and the increase has continued in historical times), and since it would be damaging to prestige to belittle the amount of goods changing hands among one's ancestors, the accounts rendered by informants are substantially correct. The idea forced itself upon me at least that with the dominant idea of coupling increase in wealth with passage of time (amply demonstrated in her passages on finance and interest), it would be contradictory to the basic time concept of potlatch and to the prestige of the present generation if the ancients equalled their descendants in amount of property. This is a minor point, on which none of the overall argument depends.

Another, and more serious, shortcoming is imposed on this author by her sources. She follows the literature in saying (p. 107) that Kwakiutl war 'did not contribute to political or social cohesiveness,' after citing at least six examples in which it obviously had. As she herself points out in another passage, the motive for war and its concomitant social groupings are very different things. Though Kwakiutl warfare may not have occasioned a grouping of persons which was stable in other social contexts, consideration of warfare led to a discussion of groups. Composition of these groups is never adequately set forth (nor is that of groups to which status in the potlatch system is said to adhere). It seems likely that the political and jural structures of Kwakiutl society are closely connected with the potlatch, if for no other reason than the connexion between potlatch and warfare which has been ably demonstrated on other grounds.

These comments are not made so much in criticism of this monograph as in the hope that Dr. Codere herself, with her obviously exhaustive knowledge of the Kwakiutl, can carry out the modern field study which she shows to be so necessary for filling in the sociological lacunae in Kwakiutl literature.

PAUL BOHANNAN

## CORRESPONDENCE

**Race.** Cf. MAN, 1950, 220 and 1951, 28-32, 93-96, 101, 122 and 151

**180** SIR.—The U.N.E.S.C.O. Statement on Race rightly claims that 'equality as an ethical principle in no way depends upon the assertion that human beings are in fact equal in endowment.' Yet the purpose and content of the statement as a whole appears to be in direct contradiction to this claim. An attempt to base judgments of value upon scientific theories inevitably incurs certain risks: the devil is notoriously fond of quoting

scripture. Hence it may be wise to elaborate this point in the more definitive statement to be issued. The argument may be summarized as follows:

(1) It is not denied that differences in physical appearance exist between groups of men. The vexed question whether such differences are correlated with differences in mental capacities and personal qualities can hardly be answered with the evidence so far available. Argument on this topic is apt to degenerate into sterile



debates on the relative importance of heredity and environment. Indeed, it may well be doubted whether a problem of this sort is capable of a satisfactory scientific solution.

(2) Attempts in the analysis of racial differences carried out so far tend to equate intelligence with the psychological make-up as a whole. But this ethnocentric emphasis on intelligence does not justify the classification of races on this basis. Intelligence is only one of a large number of valuable and important mental or personal qualities. How are their relative values to be assessed?

(3) Difficulties of this kind indicate that it may be well to shift the focus of theoretical interest from attempts to refute racial prejudice to the concrete and detailed analysis of the harmful effects of racialism, as regards not only its victims, but also its adherents.

(4) But considerations of this sort cannot justify the concept of equality as a moral or political demand. Even if one could 'prove' the equality of races, racist views would not necessarily be affected. After all, it is quite possible to refuse to listen to rational argument or to deny its validity as an imperative to action. In fact, anti-intellectual and anti-rationalist views generally form an important part of the racist *Weltanschauung* as a whole. Conversely, were racial inequality to be 'proven,' one may hope that many of us would still demand equality of treatment for all men.

(5) In sum, statements of value cannot logically be derived from statements of fact. Rousseau, in his *Origin of Inequality*, says that there are two kinds of inequalities, a natural one, arising from different endowments, and a conventional one, reflecting the sentiments and prejudices of men. To discuss whether there is any essential connexion between the two is, in effect, to ask whether those who command are necessarily better than those who obey. 'A question fit, perhaps, to be discussed by slaves in the hearing of their masters, but highly unbecoming to reasonable and free men in search of the truth.'

A. T. CAREY

London

#### Maine on Morgan

**181** SIR.—The letter reproduced below was found pasted inside the cover of the first edition of Lewis H. Morgan's *Ancient Society*, which was in Tylor's Library. It was written by Maine to Tylor, from the Athenæum Club on 27 May, and someone, presumably Tylor, has pencilled in the year—1878. I think it may be of interest to readers of MAN.

Institute of Social Anthropology, E. E. EVANS-PRITCHARD  
Museum House, South Parks Road, Oxford

My dear Tylor,

I received a letter from you here yesterday which I destroyed too soon to observe the date. But I imagine that it must have been written some time ago, since apparently the man for whom you asked me to vote has been already elected.

I look forward with much interest to the paper on Morgan. His book is certainly most curious, but there is something unsatisfactory about the man. I wrote to him at his own invitation the other day and put to him a very simple and definite question, and now I have a letter of four quarto pages complaining of the 'tendency of your (my) mind to definiteness' and of the difficulty of answering the question, but never answering it after all. Perhaps, as I hear is now the case with McLennan, he lives in fear lest anybody should get scent of his supposed discoveries.

Very truly yours,  
(Signed) H. S. Maine

#### Domestication

**182** SIR.—In Bertrand Russell's recent *Unpopular Essays* we read (p. 165): 'The taming of domestic animals, especially the cow and the sheep, must have made life much pleasanter and more secure. Some anthropologists have an attractive theory that the utility of domestic animals was not foreseen, but that people attempted to tame whatever animal their religion taught them to worship. The tribes that worshipped lions and crocodiles died out,

while those to whom the cow or the sheep was a sacred animal prospered. I like this theory, and in the entire absence of evidence for or against it, I feel at liberty to play with it.'

The known distribution of the ancestors of our domestic animals affords, of course, strong evidence against this surprising theory; it would be interesting to know who perpetrated it.

Usk, Monmouthshire

RAGLAN

Note

Lord Russell informs the Hon. Editor that the suggestion was made by Bagehot, 'but not very seriously'.—Ed.

#### Trade Ornaments and Goods: Their Archaeology and Ethnology

**183** SIR.—I am studying the archaeology and ethnology of trade goods and trade ornaments (particularly those sent from Europe and designed as acceptable to primitive communities), with special attention to their historical dating, manufacture, diffusion and influence on the art styles of the recipients.

I would be grateful if readers of MAN who are interested in this subject would let me know of any material of special significance that they have noted in museums or private collections, or in little-known references (especially in the writings of early travellers). The Pitt Rivers Museum now has an exhibition case specifically devoted to the subject. Its further detailed study is important for the wider field, now increasingly requiring attention, of the archaeology of the recent past in primitive communities, particularly in Africa. It also throws much light on primitive trade, and illustrates the routes used, and the varied and often remarkably strange ways of diffusion of ideas and objects. Mr. A. J. Arkell's valuable researches on the trade in Cambay Beads (*Antiquity*, 1936, pp. 292-305), and on Tuareg ornaments, their connexion with India and the modern Czech imitations of them (*J. R. Anthropol. Inst.*, Vol. LXV, 1935, pp. 297, 307) are instances of how much can be discovered.

There are, of course, such contemporary records as early bills of lading (e.g. those for goods for trade at Tolia Point, July, 1515, etc.; see J. W. Blake, *Europeans in West Africa, 1482-1560*, Hakluyt Society, Series II, Vol. LXXXVI, pp. 118-121). But excavation will always be required, in the end, to get to grips with the details of the material culture of the recent past. This has been successfully demonstrated in the most convincing manner by Mr. Kenneth Kidd's treatment of the finds and trade imports from his excavation of a Jesuit missionary settlement in Canada with a known and finite occupation from 1639-1649 (see *The Excavation of Ste. Marie I*, Royal Ontario Museum of Archaeology, 1949). With such excavations, whole classes of trade goods can be dated and used to construct the chronology of indigenous cultures when found in association with them (just as the Roman Arretine pottery from the trading station at Arikamedu near Pondicherry, excavated by Professor R. E. M. Wheeler, is being used to date the pottery of neighbouring Indian cultures).

In the meantime, however, a great deal of useful preliminary work can be done, in establishing more details of fact and distribution and chronology for the trade material, ancient and recent, in existence in museums or publications but of uncertain provenance or date.

Pitt Rivers Museum, J. S. P. BRADFORD  
University Department of Ethnology, Oxford

#### Corrections: MAN, 1951, 96 and 97

**184** Dr. Trevor asks that two slips in his reply (MAN, 1951, 96) to Mr. Dover's letter on Race should be corrected. In line 21 of the second paragraph, 'scenasiologists' should be so spelled; and in note 8, the reference to the review of Dr. Calman's book should have been given as MAN, 1950 (not 1949), 155.

By an editorial oversight which is much regretted the drawings numbered (iii) and (iv) in fig. 1 of Mr. Dark's article on Bush Negro calabashes (MAN, 1951, 97) became transposed, and readers are asked to make the necessary correction. Reference to fig. 3 or to Plate E will in any case make it clear which is the spherical intercept.







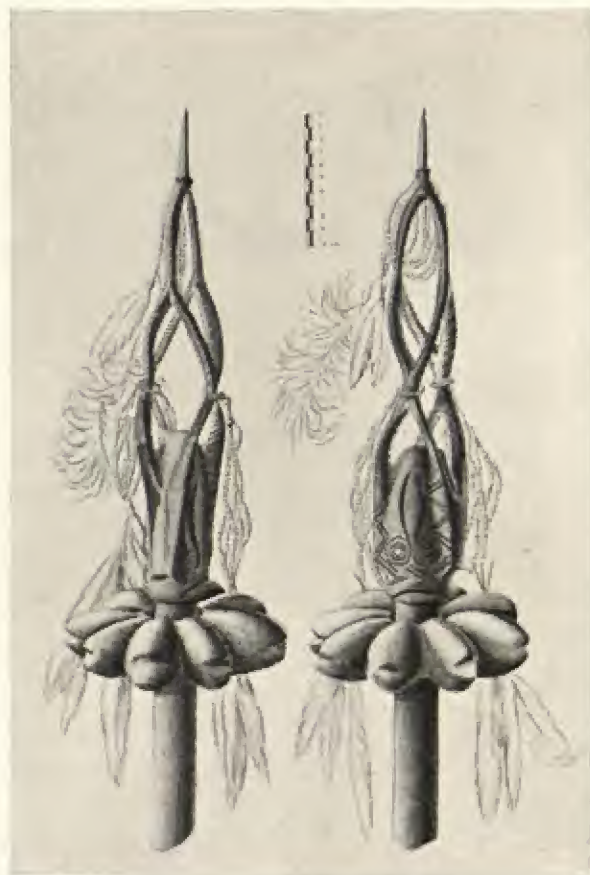
(a)



(b)



(c)



THREE BARATU CLUBS FROM CENTRAL NEW GUINEA

*From drawings by O. La Bella*



# ON THE 'MYSTERIOUS' BARATU CLUBS FROM CENTRAL NEW GUINEA\*

by

VINIGI L. GROTTANELLI

Rome

185

The stone-headed clubs from central New Guinea which form the subject of these notes are, I believe, almost unknown to many ethnologists. As shown by the figures, their characteristic feature lies in the elaborate wooden fretwork surmounting the head. These clubs have not escaped the attention of specialists, but a curious sequence of misunderstandings among the latter as to the nature of their workmanship has created a sort of confused legend about them. This, and the fact that they appear to be of a unique type, make it worth while to sum up what is known about them, and to give for the first time an accurate reproduction of them. The drawings have been made by Signor O. La Bella from the originals in the Museo Preistorico-Etnografico L. Pigorini in Rome.

The first four of these clubs (Plate Ha-c) were collected by the Italian explorer Luigi M. d'Albertis on 16 July, 1876, in a village on the middle Fly River, just south of 7° S. lat. In his book<sup>1</sup> he gives a rather rough sketch of them with the following brief comment, which I translate literally: 'A war weapon, which is perhaps also used at great festivals as a mark of authority, and which Maino calls *baratu*, will give an idea of the capacity of these savages for carving wood and working in very hard stone. A sketch of them will be of more value than any description.' In the English translation, published the same year, the word 'wood' was accidentally omitted, the sentence thus reading 'the capacity of these savages for carving and working in very hard stone.'<sup>2</sup> This mistranslation must have led readers to assume that not only the club head, but also the upper part of the shaft, was made of stone—the latter being by far the more elaborately carved of the two, as well as the most conspicuous in the figure. A. C. Haddon seems to have taken this for granted, as he wrote of the 'perforated ornamental tops wonderfully carved in hard stone,'<sup>3</sup> adding at a later date, 'The artists who made these objects were probably the most skilled workers in stone of whom we have any record in this quarter of the globe; indeed I do not recall any stone work of its kind in any part of the world or at any period.'<sup>4</sup> De Clercq and Schmeltz do not seem to have been taken in, as they mention the '*boven den steen vooruitstekende gedeelte van den steel*,'<sup>5</sup> but Finsch himself later voiced his admiration of the *baratu* with their

\* With Plate H and a text figure

'besonderem wunderbar durchbrochenen gearbeiteten Aufsatz aus Stein am Kopfe,'<sup>6</sup> and nobody seems to have questioned the accuracy of these statements until Haddon himself many years later returned to the subject, this time describing the clubs as having 'carved wooden tops.'<sup>7</sup> Far from having settled the problem, this tacit admission of his previous error resulted in creating an even worse confusion. Höltker, who has recently dedicated two whole pages of a fundamental article of his to the by now mysterious *baratu*, still assumes the existence of a type of club with pierced stone top.<sup>8</sup> He is puzzled by the 'striking uniqueness' of these weapons, for which (no wonder!) he declares himself unable to find any parallel, and seems inclined to doubt, if not the existence of the clubs themselves, at least the '*Richtigkeit der Angabe*'; but these doubts must vanish, he adds, 'as Haddon himself was able to see and study the objects in the Rome Museum.' This, indeed, is the most surprising aspect of the whole matter, since it is substantiated by Haddon's own words, written at the time of his first account of the clubs; he says that he even made a sketch of one of the *baratu*, and this can be seen in his volume.<sup>9</sup> Hereupon Höltker dismisses his suspicion of the clubs having been conjured up Cagliostrowise by d'Albertis, or simply being a mistake. The only possible suggestion, he writes, is 'that the term *baratu* generally indicates a type of club from the middle Fly River, bearing above the club head an ornamental slab of wood or stone, and that within the category . . . the ones with the stone top are a rare exception. Be this as it may, the mysterious *baratu* of the Fly River remain insufficiently accounted for.'<sup>10</sup> Two questions, according to Höltker, remain unsolved: first, how it happens that since the days of d'Albertis no other such object has been reported—perhaps, he ventures to suggest, d'Albertis bought all available specimens? Second, how can this unsurpassed technical skill in stone-carving be accounted for among the Fly River tribes?

The second question of course no longer arises, as the pierced stone slabs never existed. A mere glance at the original Italian text of d'Albertis, or indeed at the clubs themselves, should long ago have been sufficient to dispose of the 'mystery.'

The first question calls for a longer answer, though one cannot agree with the terms in which it is formulated by

## NOTES ON PLATE H

All three pieces were collected by L. M. d'Albertis and are in the Museo Preistorico-Etnografico L. Pigorini, Rome. (a) No. 2014. Length of club, 154 cm.; of carved top, 575 mm. (b) No. 2012. Length of club, 135 cm.; of carved top, 375 mm. Patterns in yellow, red and black on light brown wood. (c) No. 2010. Length of club, 112 cm.; of carved top, 360 mm. The fourth d'Albertis piece (No. 2013), of very similar appearance to (b), measures: length of club, 140 cm.; of carved top, 460 mm. In all four, the openwork top and the shaft are carved from one piece of wood.



Höltker. First, it is clear from d'Albertis' diary that the four clubs were not *bought* (they would probably not have been for sale), but *found* in a village from which all natives had hastily fled on the previous day, scared by the sudden appearance of the explorer's steamship. This village d'Albertis named Villaggio dei Cocchi (Cocos village). Further, even if the four specimens of the clubs which he took away were the only ones existing in the tribe at the time, nothing prevented the people from making more of them. The two reasons for the rarity of the *baratu* are indeed of a quite different nature: they were used in connexion with head-hunting, a custom which has been rapidly declining in Papua since the end of last century; and they were customarily destroyed by their owners when they had once been used. This applies, at any rate, to the nearly identical *parasi* weapons from the Keraki and Wiram areas, described by F. E. Williams in 1936. These were used in delivering the first blow to the enemy on a head-hunting expedition, being 'shattered over the heads of the victims before they are actually clubbed,' and then immediately thrown away; 'no man . . . would have the face to return from an expedition bearing a *parasi* intact.'<sup>9</sup>

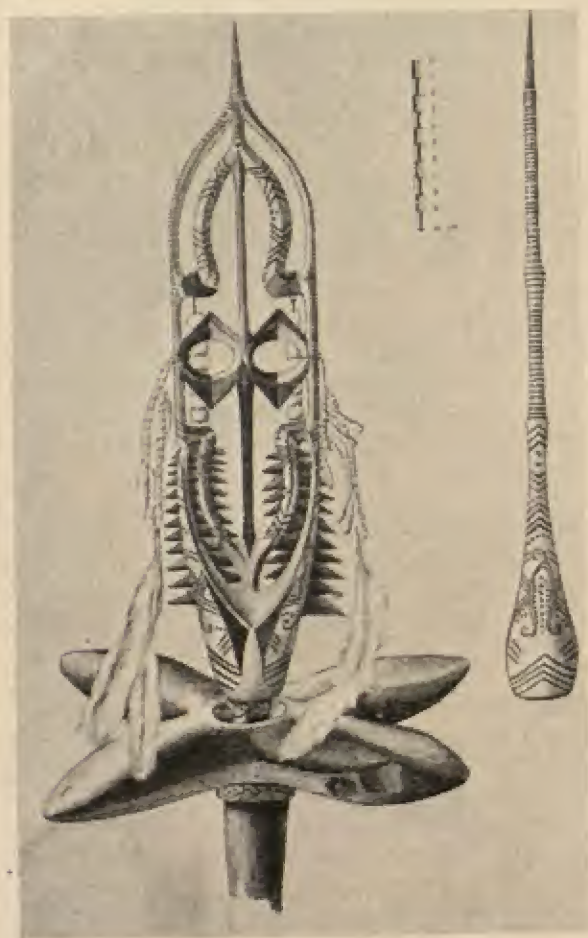


FIG. 1. BARATU CLUB IN THE ROME MUSEUM  
Catalogue No. G/3231. Length of club, 152 cm.; of carved top, 370 mm.;  
width of top, 255 mm. Top is carved separately and fitted to shaft.

The similarity in the general design and fretwork technique, and even in the names, unquestionably shows that the clubs from Cocos village and Williams's *parasi* belong to one and the same class of weapons. There is, however, one noticeable difference between them: the latter lack the stone head altogether; they are a simplified, purely magical weapon, whereas the *baratu*, apart from their symbolical use, might also have served for killing the victim.

Williams was not the only European who came across these weapons after d'Albertis: two more specimens, at least, are known from European museums. One is in the Leiden Museum.<sup>10</sup> It is of coarser make, and entirely of wood, including the disc-shaped imitation of the stone head. It was collected with the name of *hayam* among the Tugeri (Marind-anim), according to Giglioli by W. de Jong in Merauke.<sup>11</sup> The other, now in the Rome Museum (fig. 1), was collected by S. MacFarlane on the upper Fly in 1885; Giglioli bought it for his private collection from E. Gerrard in London in 1886. In his catalogue he adds the following notes: 'It belonged to the savage Sahiraka Tugeri living west of the Bensbach River, who in groups of 800 to 1000 have been known to invade the country between the latter and the Fly, slaughtering and destroying.'<sup>12</sup> Further information, in Giglioli's own handwriting, is found on the label attached to the club. It says: 'My friend A. C. Haddon saw a club of this form, with a rough handle, at Mer, Torres Straits Islands; it is known, however, that such clubs are imported from Daudai, i.e. New Guinea.'

A further specimen, quoted by Finsch in connexion with the *baratu*, comes from the Morehead River. The disc-shaped stone head is here again surmounted by a tall ornamental slab, apparently decorated with parallel horizontal coloured stripes, but not actually perforated.<sup>13</sup>

Giglioli also quotes a Kiwai name for these clubs, *tumanabala*, though he does not mention the source of this information.<sup>12</sup> It is disappointing to find no mention of them in Landtman's paper on Kiwai warfare<sup>14</sup>; perhaps, here as elsewhere, the handsome weapons were no more than occasional imported objects, valued as masterpieces of handiwork. A specimen collected by Mr. Zimmer near Lake Murray was being used as a dancing club, *gigi*.<sup>15</sup>

The great difficulty of obtaining full information about these weapons is illustrated by the experience of Williams, the only author who has at all succeeded in recording their real significance. He too, though apparently not acquainted with previous literature on the *baratu*, calls them 'mysterious,' as even his native informants were 'unable to suggest any reason for this apparently futile practice of striking the victim with a flimsy wand which merely broke to pieces on his head.'<sup>15</sup> He was probably right in attributing this ignorance to the fact that the *parasi* were not properly a Keraki weapon, but had been borrowed from some other tribe (the Wiram, he suggests) 'without understanding of their rationale.'

This brings us to the last and most important question, to what original tribal group must these clubs be assigned. A comparative technological study of the carving technique



and of the ornamental patterns in colour and design lies beyond the scope and space of this short note. Broadly speaking, affinities point to the north-west, where the wooden fretwork technique (*korwaar* 'shields,' headrests, canoe ornaments, etc.) is far more advanced than in the rather massive art of the eastern districts. Unfortunately, we have as yet no such standard work on this western art as, for example, Miss Reichard's book on Tami and other Melanesian areas, and our knowledge of the culture pattern of Dutch New Guinea is still too scanty to make a comparison possible on these lines only.

But we do know that head-hunting, from which our clubs cannot be dissociated, is in Western Papua 'a comparatively recent addition to the cultural heritage, having spread from Dutch New Guinea.'<sup>16</sup> The owners of the *baratu* themselves, indeed, were a party of head-hunting marauders probably from the west. In the Villaggio dei Cocchi, which had not been there when he had passed the spot only 40 days earlier, d'Albertis found a number of bundles each containing one, two or more human heads painted red, apart from the single stuffed head which Haddon later selected as the characteristic feature of his Kabiri culture area.<sup>17</sup> The ethnographic data which d'Albertis noted here and the objects he collected (such as mat capes, manufactures of plaited human hair, the fine barbed bone-tipped arrows, absence of pottery and apparently also of shields, etc.) coincide almost exactly with the corresponding Tugeri elements described twenty years later by Sir William MacGregor, who defeated a party of these marauders on the Wassi Kussa in 1896.<sup>18</sup> To give just one significant detail, the double scroll pattern on the *baratu* tops, and on the paddles and arrows from Villaggio dei Cocchi, is identical with that on the Tugeri arrows collected by MacGregor and A. Giulianetti on that occasion, now in the Rome Museum.<sup>19</sup> The fact that the *baratu* belonged to a Tugeri (Marind) group had been realized by Giglioli as early as 1904 or 1905,<sup>20</sup> and a suggestion to this effect was made by Haddon in 1924;<sup>21</sup> the comparisons I have been able to make establish the fact beyond doubt. The distribution of these clubs, as it appears from the literature quoted above, accords with this, including as it does localities that till the end of last century used to be periodically raided by the Marind, and which according to the traditions recorded by Wirz were formerly parts of their own homeland.<sup>22</sup> On the other hand, apart from the Leiden and the Giglioli clubs, which are the only two originally labelled Tugeri, I know of no other records of such clubs from the Marind proper.

The Marind-anim however, or at least some of their groups, have a special type of ceremonial weapon which I think can be considered equivalent to the *baratu* or *parasi*: it is a spear bearing in its centre a flat oval slab carved out of the shaft, and perforated with ornamental patterns in a style strongly reminiscent of that of the *baratu* tops.<sup>23</sup> Father J. Boelaars, M.S.C., Merauke, was kind enough to inform me that the Yaqai of Mapi area (a Marind-speaking tribe) use these spears on their head-hunting raids: just before launching the attack, a warrior hurls (and breaks?) one of them towards the enemy, in order to ensure the

success of the enterprise. Now the same type of spear was used by MacGregor's Tugeri in 1896, whom we have seen to be closely allied to the owners of the *baratu* met by d'Albertis. One can thus hardly fail to see a link between these two classes of weapons, clubs and spears, made by groups of the same stock, decorated in the same fretwork technique which is otherwise unusual, and both used for some magico-symbolical purpose connected with head-hunting. The 'mystery' of the *baratu*, as Höltker saw it ten years ago, is solved in its technological aspect, but only to be replaced by the far more complex problem of its far-reaching implications in the ideological sphere.

## Notes

- <sup>1</sup> L. M. d'Albertis, *Alla Nuova Guinea*, Turin, 1880, pp. 386f.
- <sup>2</sup> L. M. d'Albertis, *New Guinea*, London, 1880, Vol. II, pp. 135f.
- <sup>3</sup> A. C. Haddon, *The Decorative Art of British New Guinea*, Irish Acad., Cunningham Memoirs, No. 10, Dublin, 1894, pp. 77f. and Plate V.
- <sup>4</sup> A. C. Haddon, 'A Classification of the Stone Clubs of British New Guinea,' *J. R. Anthropol. Inst.* Vol. XXX (1900), pp. 244f.
- <sup>5</sup> F. S. A. De Clercq and J. D. E. Schmeltz, *Ethnographische beschrijving van de West- en Noordkust van Ned. Nieuw-Guinea*, Leiden, 1893, p. 230.
- <sup>6</sup> O. Finsch, *Südseearbeiten*, Hamburg, 1914, p. 256.
- <sup>7</sup> A. C. Haddon, 'Migrations of Cultures in British New Guinea,' *J. R. Anthropol. Inst.*, Vol. L (1920), pp. 242f.
- <sup>8</sup> G. Höltker, 'Einiges über Steinkülenköpfe und Steinbeile in Neuguinea,' *Anthropos*, Vols. XXXV-VI (1940-41), pp. 713-15.
- <sup>9</sup> F. E. Williams, *Papuans of the Trans-Fly*, Oxford, 1936, pp. 265f.
- <sup>10</sup> Rijks Ethnographisch Museum te Leiden, *Verslag van den Directeur . . . 1902-03*, 's Gravenhage, 1904, p. 61 and Plate VIII (No. 1392/63).
- <sup>11</sup> Label attached to No. G/3231, M.P.E., Rome.
- <sup>12</sup> *La collezione etnografica del Prof. Enrico H. Giglioli*, Città di Castello, 1911, Vol. I, pp. 269f.
- <sup>13</sup> *Op. cit.*, p. 256, n.1: *Annual Report for British New Guinea 1895-96*, Brisbane, 1897, Plate 15.
- <sup>14</sup> G. Landtman, 'The Magic of Kiwai Papuans in Warfare,' *J. R. Anthropol. Inst.*, Vol. XLVI (1916), pp. 322-33.
- <sup>15</sup> Williams, *op. cit.*, p. 268.
- <sup>16</sup> A. P. Elkin, 'Man and His Cultural Heritage,' *Oceania*, Vol. XX (1949), p. 16.
- <sup>17</sup> A. C. Haddon, 'The Kabiri or Girara District, Fly River, Papua,' *J. R. Anthropol. Inst.*, Vol. XLVI (1916), p. 352. See also S. Chauvet, *Les arts indigènes en Nouvelle Guinée*, Paris, 1930, p. 217 and fig. 28, and H. W. Krieger, 'Design Areas in Oceania,' *Proc. U.S. Nat. Mus.*, Vol. LXXIX (Washington, 1932), art. 30, plate 27. Cf. P. Wirz, *Die Marindanim von Holl. Süd-Neu-Guinea*, Hamburg, 1925, Part III, p. 59.
- <sup>18</sup> *Annual Report* (see note 13 above), p. 55ff.
- <sup>19</sup> Cat. nos. G/15296-305, G/15180-85, etc. Compare with the patterns in M. Laroche, 'Notes sur quelques ornements de pirogue de la Nouvelle Guinée Hollandaise,' *Journ. Soc. Océan.*, Vol. V (1949), p. 114.
- <sup>20</sup> Introduction to A. C. Holmes, *In Primitive New Guinea*, London, 1924, p. 9.
- <sup>21</sup> G. R. Le Hunte in *Ann. Rep. Br. N.G.* 1899-1900, Brisbane, 1901, pp. 26f.; A. C. Haddon, 'The Tugeri Head-hunters of New Guinea,' *Int. Arch. f. Ethn.*, Vol. IV (1891), pp. 177f.; P. Wirz, *Die religiösen Vorstellungen und die Mythen der Marind-anim*, Hamburg, 1922, pp. 24f.
- <sup>22</sup> Unpublished photographs by P. Drabbe, M.S.C., Merauke. Two spears of this type, from 'S.W. New Guinea,' are in the Pontificio Museo Missionario-Etнологico, Rome, Nos. AU/494/A and C. Two more, from the Tugeri, are reproduced in *Ann. Rep. Br. N.G.* 1895-96, Plate 10; see MacGregor's description, *ibid.*, p. 56.



## A POT FROM NAG HANISH NEAR MERSA MATRUH\*

by

R. H. PARKER

Tauranga, New Zealand

186 Among the enormous mass of sherds which in places almost paves the Marmarican coastal plain one occasionally encounters chips of roughly hand-made pots devoid of wheel marks and often decorated with deeply incised designs immediately under the rim. Fragments of a grey ware of this kind are to be found near Baggush and Sidi Hanish and a somewhat similar ware with the outer surfaces of the clay burnt brick red occurs in the same area and also at least as far eastward as Burgh el Arab. The ancient use of undecorated hand-made pots at Mersa Matruh was established by Oric Bates.<sup>1</sup>

In the autumn of 1941 I found near Nag Hanish south-east of Mersa Matruh a number of small sherds of grey incised ware lying together. It was apparent that they belonged to a single vessel. About a year later the fragments were reconstructed in the Egyptian Museum, where they were found to comprise one side of a large pot (fig. 1).



FIG. 1. RECONSTRUCTED FRAGMENTS OF A LARGE POT FROM NAG HANISH, LIBYA

The pot was examined both before and after reconstruction by Mr. Guy Brunton, and I had previously shown one or two pieces to Mr. Alan Lucas, who was unable to suggest a possible source of the material.

#### *Description of the Pot*

The fragments when found were lying on the surface of a flat space about 200 yards south of a small cistern on the very edge of the main escarpment and about two or three miles west of Nag Hanish. The area seemed to be undergoing fairly heavy wind erosion and the pot could not have been exposed long. It was, however, much weathered, and the surface pitted, probably by blown sand. Lying under one fragment was a minute, intensely blue, glass bead.

The material of the pot had a curious purple tinge in the

\*With a text figure

fracture which I have not noticed in any other Libyan sherd.

After reconstruction it could be seen that the lower part of the pot was discoloured by fire and the interior showed patches of a sooty substance. The modelling of the vessel was crude in the extreme and the thickness of the material very variable. The roughly incised design under the rim was built up of two conventional elements repeated serially and had presumably encircled the pot. In places the incisions contained traces of white and the design had possibly been filled in originally with a white pigment (as was done after reconstruction to facilitate photography).

#### *Discussion*

Mr. Brunton, in a letter dated 8 February, 1942, said that the pot was 'generally Mediterranean' and that he thought that it was probably 'Predynastic or at least Archaic' in date. He considered that the glass bead was likely to be an accidental intrusion.

The bead was lying immediately under a piece of the pot with no earth between and I am certain that they must have been contemporary. Small glass objects have been reported from Egypt as early as the late predynastic, and although the dating of these has been disputed it is possible that no contradiction need arise.<sup>2</sup>

In regard to the decorative design, the sign seen in the middle of fig. 1 is found in the Tifinagh script as a variant of the form with all the component lines straight, which alone occurs in the West Libyan,<sup>3</sup> and from this point of view it could probably be regarded as an Eastern Libyan form. It is worth noting here that another sign, consisting of two upright parallel lines, one or both having a barb-like projection at the top, which I have seen on sherds of the grey incised ware at Baggush and Sidi Hanish, is an element in the West Libyan script and that somewhat similar signs are found as rock glyphs in Marmarica, where they were probably used as tribal marks.<sup>3</sup> The sign seen in the middle of fig. 1 also occurs in one of the 'inscriptions' reported from Dhufar in southern Arabia;<sup>4</sup> these are said in the text to be 'pre-Arabic, possibly early Ethiopic,' though the ground for this is not stated. The fact that at least one of these Dhufar signs has persisted in the area down to modern times as a camel brand of the Sa'ar tribe<sup>4</sup> raises the possibility that the presence of the sign now under discussion in Marmarica and in the Tifinagh script may be due to Arab influence in the period after the invasion of A.D. 641. However, the apparent antiquity of the pot and the existence of definite and relatively early Libyan affinities for the decoration combine to render it rather unlikely. Moreover, a detailed comparison of the Dhufar signs with the letters of the West Libyan script



reveals a surprising number of very close resemblances. It is therefore not necessary to invoke late Arab influences to account for the presence of any particular one of the Dhufar signs in North Africa.

On the whole it seems reasonably certain that the pot is Libyan ware of local manufacture, though the proximity of the site to the outlet of the Mersa Matruh—Siwa road makes the local manufacture a good deal less certain. In the almost complete absence of information about East Libyan pottery it would be quite unsafe to attach any definite date to either the vessel or the bead.

### *Contribution to the Classification of Marmarican Sherds*

The following are summary descriptive notes on some types of Marmarican potsherds known to me:

#### *I. Hand-made Wares*

- A. Colour and texture probably variable; no decoration; found at Mersa Matruh; described by Bates in *Ancient Egypt*, Vol. IV (1915).
- B1. Grey colour with purple tinge in fracture; tarry texture; deeply incised designs; found at Nag Hanish; single pot only (described in this article).
- B2. Grey colour; tarry to coarse texture; deeply incised designs; Baggush and Sidi Hanish district.
- B3. Red colour; tarry to coarse texture; deeply incised designs; Sidi Hanish and Burgh el Arab; most commonly with a grey layer between the red outer surfaces.

#### *II. Wheel-turned Wares*

- C. Grey colour; rather open texture; green wash fading to white where exposed to weather; found generally.
- D1. Red colour; open texture; green wash as C; found generally.
- D2. Red colour; close texture; green wash as C; found generally.
- E. Grey colour with white flecks (probably calcium carbonate); close texture; lightly incised geometrical designs; from Sidi Hanish.
- F.1 Red burning clays give colour range from brown through chocolate, dark red, light red, to pink; the textures are very variable, except in the pink which has a very close sandy texture; as in most red wares a grey layer is often present between the red or brown outer surfaces, but is rare in the lighter shades of red.
- F6 Red colour (cross section red-pink-grey-pink-red); very close texture; from a point 20 miles south of Burgh el Arab; part of a single attractive vessel.
- G.1 Grey burning clays with a wide range of shades and textures; together with the red types above (F) these make up about 80 to 90 per cent. of the potsherds in Marmarica; these grey burning clays are probably either deficient in iron compounds capable of being converted into the red

oxide by heating, or contain appreciable quantities of calcium carbonate.

- H. Buff colour; variable texture; sometimes very lightly incised with slightly sloped parallel straight lines; found generally; most buff-coloured sherds are modern, but a few are probably older.
- I. Black colour; very close texture; from Mersa Matruh; a single sherd of a very beautiful ware.

The use of a pale green wash which has turned white where exposed to weathering is fairly common in the Sidi Hanish and Mersa Matruh areas. In a midden at Matruh I saw a single sherd of dark grey painted with a very beautiful dark green wash. One occasionally sees what may be instances of the use of a red-ochre wash resembling that employed anciently in Egypt. At el Alamein I saw a single instance of what may have been a grey slip used on red ware.

With regard to dating, we may note that types A, B1, B2 and B3 are hand-made wares, except that a few sherds of B3 show marks which may indicate manufacture on a primitive kind of wheel. It is not possible to say at what date the wheel was introduced into Marmarica, but since it presumably came from Egypt it is not likely to have been before 3000 B.C. or, on the whole, much after 2000 B.C. The manufacture of hand-made wares would continue long after the first introduction of the potter's wheel and may have persisted even into modern times in some localities, but since the A and B types seem to form a definite group, only a very few of which show signs of having been turned on a wheel, the presumption is that they are all ancient and should be dated at least before 1200 B.C. The use of green washes appears to be a local characteristic owing nothing to Egyptian or other foreign influence, and it seems probable that these wares too should be dated before 600 B.C. The greater part of the remaining types undoubtedly belong to the period 600 B.C.-A.D. 640, though of course there must have been a considerable body of sherds deposited during the Arab period. The single black sherd mentioned (type I) may be of Egyptian origin or at least indicate Egyptian influence.

#### *References*

- <sup>1</sup> O. Bates, paper in *Ancient Egypt*, 1915, Part 4.
- <sup>2</sup> A. Lucas, *Ancient Egyptian Materials and Industries*, London (Arnold), 1934, pp. 116f.
- <sup>3</sup> O. Bates, *The Eastern Libyans*, London (Macmillan), 1914, pp. 87-89.
- <sup>4</sup> B. Thomas, *Arabia Felix*, London (Cape), 1932, p. 126, Plate, and Appendix V.

## SHORTER NOTES

### IVth International Congress of Anthropological and Ethnological Sciences

**187** The Fourth Session of the International Congress of Anthropological and Ethnological Sciences will be held in Vienna in the first week in September, 1952. The General Secretaries are Dr. W. Koppers and Dr. J. Weninger, and the address of the Secretariat is Institut für Völkerkunde, Neue Hofburg, Corps de Logis, Vienna I, Austria. Full particulars will be announced later.

### XXXth International Congress of Americanists

**188** On the invitation of the Royal Anthropological Institute, accepted at the New York Session in 1949, the International Congress of Americanists will hold its Thirtieth Session at Cambridge (Eng.) from 18 to 23 August, 1952. Accommodation will be provided by one or more colleges of Cambridge University. Subscriptions for full membership will be £3 3s. Cheques should be made payable to the 'International Congress of Americanists,' and sent to the Joint Secretaries of the



Organizing-Committee, International Congress of Americanists, c/o University Museum of Archaeology and Ethnology, Downing Street, Cambridge, England. Contributions towards a Guarantee Fund and donations to the General Fund are invited. Circulars giving full particulars will be issued later this year.

The date of this Congress has been arranged in consultation with the organizers of the International Congress of Anthropological and Ethnological Sciences at Vienna (see the preceding article) so as to permit those who so desire to attend both Congresses.

## REVIEWS

### GENERAL

**The Foundations of Social Anthropology.** By S. F. Nadel. London (Cohen and West), 1951. Pp. xi, 426. Price £1. 5s.

**189** Whitehead writes in *Science and the Modern World*: 'If science is not to degenerate into a medley of *ad hoc* hypotheses it must become philosophical and must enter into a thorough criticism of its own foundations.' It is this task that Professor Nadel has set himself in an ambitious book which is concerned with 'the logical premises that underlie our knowledge of societies (whether they be primitive or otherwise) and with the prerequisites, conceptual and technical, of any inquiry meant to lead to this knowledge.' It would be impossible to summarize shortly the contents of this book, but it may be considered as two parts: a body of definitions and discussions of anthropological concepts and issues; and an attempt to construct a coherent system of sociological understanding. The second is the major and most novel part of the book.

Professor Nadel believes that social anthropology is ready to claim a place among the sciences; and, as he considers it a science in so far as it explains, he tries to show how and on what grounds it can explain social phenomena. The subject matter of anthropology is action, or behaviour that is aim-controlled: 'action' is motor events in individuals that are intended to have consequences. Behaviour is sociologically relevant only if it is aim-controlled or enters into aim-controlled action patterns, if the behaviour can be expressed in terms of a task. 'There must be, somewhere in the task pattern, consciousness, and somewhere in its activation, purpose. Without these two factors there can be no social understanding.' Ideally, the different methods of explanation in the science of society should be compatible: a sociological 'law' should be translatable into the laws obtaining for physiology, psychology, biology, etc. Explanation is description plus the formulation of some kind of 'fitness' or 'requiredness' in the discovered regularities, and in social regularities there are three kinds of fitness: logical consistency; purpose and means-to-end relations; and mechanical causality. The first two do not require extra-social verification, the last tends to do so.

In the hierarchy of the sciences every social fact is at any given moment a complex of psychological, physiological, and innumerable other processes discoverable on yet deeper levels of analysis, and can be broken down to these events and processes. Sociological and psychological phenomena interact, thus turning the hierarchy of levels into true causal relations between the levels. Social facts arise because the actors have had certain intentions or felt the efficacy of certain mental events; and they have intentions and other mental events because certain social facts exist. Social facts, then, are 'emergents' from the order of things that we call psychological and organic; and the problem of finding mechanical causal requiredness or fitness in social phenomena can be illuminated by a move to lower levels of analysis. In such a move there are three assumptions that the student of society must make: the acceptance of mental energy, the efficacy of psychological linkage, and the efficacy of innate action potentials in the genesis of actions.

Social behaviour patterns are intentional and task-like; any task reveals on psychological analysis an inherent tension set up by it and carrying its execution. Every social behaviour pattern, therefore, contains something in the nature of an energy which holds it together and accounts for its activation: in every statement on intentional behaviour we presuppose the operation of mental energy, and thus definite regularities governing the dynamic 'functioning' of psychological processes (when we speak of behaviour motivated by affection, sexual attraction, or other emotions, sentiments, or

thoughts, 'we are using terms meaningless without the assumption of mental energy'). There are two 'laws of even and uneven mental energy' and two mechanisms governing its flow—repression-release, and 'psychic economy': we assume that they are visible both in individual and group behaviour, 'yet we are unable to identify and distinguish them clearly in the latter.' These laws account for the linkage of social facts.

Innate action potentials are the tensions and pressures projecting into awareness and requiring to be relieved by action. They may be specific (hunger, fatigue, sex) or generic (released in a whole class or series of modes of behaviour). The action potentials with which we are concerned start long-range cycles of behaviour in that they admit of delayed consummation. 'The generic potentials stand in an intricate and partially obscure interrelation, which makes their precise definition difficult.' Also, they seem characterized by an inherent ambivalence and polarity: three such pairs of drives are pleasure-displeasure, equilibrium-tension (striving for tension is a dynamic component in its own right), and conformity-shame (the conformity action potential is ascribed a very deep foundation in the mental sphere). There is however no means of calculating the 'normal' strength of drives, or of estimating, for example, whether there is enough pleasure or excitement in social life, or how far these drives have been denied their fair measure of self-expression. But we can say that once an institutionalized occasion bringing innate action potentials into play is offered, the generic drive so actuated must be assured the satisfaction peculiar to its genus.

The two main points, then, on which the whole explanatory scheme rests are Purpose ('we are forever on the lookout for purposes which must somehow be realized and which account for the varying modes of behaviour as for so many means appropriate in different circumstances') and the 'emergence' of social from psychological and physiological facts.

When the author states that without the recognition of purpose there can be no social understanding he writes: 'I have stated this as a postulate, and such it is.' But he provides no evidence of a compulsive character to show the validity or the pragmatic value of introducing purpose as a major category in sociological explanation, or to show that he is not making 'a false assimilation to technical acts' in forever being on the lookout for purposes. Also, in spite of its importance, the section on Purpose is inconclusive and not quite clear. This is further complicated by a Whiteheadian passage towards the end of the book: 'Society and culture are made and worked by man. May we not assume that they are made and operate for man? The Great Engineer is merely Man in the abstract, and the Intelligence at the back of all things social, the Human Mind writ large. . . . This Intelligence is not in the intentions of which the actors are aware nor in the narrow-range rationality which makes them link action with action and move from event to event; at least, this Intelligence enters into their actions only in an irregular and fragmentary fashion. It is we who feel impelled to piece the design together, to guess at the Intelligence, and construe an ulterior Purpose' (p. 368). These capitalized beliefs are obviously very important to the author: it is a pity they are so mystifying.

Similarly with his plea for the admission of psychology in explanation. This rests on the postulate that social facts are in some way 'emergent' from psychological and organic facts; that the former are not merely implicit in the latter; that there is some attribute of the social fact which makes it something more than merely translatable into terms of psychology or physiology. This is



not proved, and there is the same lack of decisiveness and clarity in the section dealing with it.

Professor Nadel does not demonstrate in defence of either of these major postulates how they are of explanatory value to the extent that he wishes to advocate, but they must both rely for acceptance on empirical grounds. There thus seems to one humble reviewer no logical necessity to accept either postulate on which the proposed explanatory scheme depends; and their pragmatic value remains undemonstrated. But, successful or unsuccessful, the author is describing not what might be commonly accepted on examination as the foundations of social anthropology but what he thinks they ought to be. As far as this, the major part of the book, is concerned the title is a misnomer; but in it lies much of the book's great interest, for it is the declaration of faith of an eminent anthropologist, and of considerable value and fascination as such.

More valuable and acceptable are the closely analytical discussions of the concepts and issues of anthropology; such as anthropology and its relation to science and history, sociological laws, society and culture, personality, and a great number of other definitional and logical exercises. These are thorough and stimulating (see, for ex-

ample, the devastating analysis of some of the Wilsons' *Social Change* propositions on p. 102), and so widely ranging that it is difficult to imagine a future anthropological discussion in which someone will not say at some point: 'Well, Nadel says . . .' The author is extremely widely read, and it is most refreshing to find Carnap, Russell, Wisdom, and Wittgenstein in the bibliography (even though Whitehead seems to have been a stronger influence) instead of the usual drearily familiar list.

It is a book that will provoke the widest possible controversy: it is erudite, consistent, and stimulating, and altogether one of the most important theoretical works in anthropological literature. Even where it is a failure (as some will consider the plea for purpose and the systematic scheme of explanation a failure) it is an important failure. It is impossible in a review to give an idea of the breadth of its scope or of the patient and scholarly exploration of concepts and explanatory possibilities: every anthropologist will have to read a work of such provocative ambition. But social anthropology still needs rigorous philosophical analysis: probably we need a philosopher to do it and not a philosophical anthropologist, even a brilliant one.

RODNEY NEEDHAM

## EUROPE

**La España Primitiva.** By Luis Pericot García. Barcelona (Editorial Borna), 1950. Pp. 374. Price 75 pesetas

**190** This book fills a need that has been very pressing for many years—the need for a compact, comprehensive, clear and cheap summary of Iberian prehistory. It is the sixth volume in a series of historical works edited by Dr. Pericot himself, and provides, for a pound, an extremely well balanced and well illustrated account. The treatment is chronological and is divided into three sections which correspond to the three main divisions of West European prehistory, namely, first the Palaeolithic and Mesolithic, secondly the Neolithic and Bronze Age, and thirdly the epoch of the Celts and Iberians which in Spain at least corresponds to the Early Iron Age.

Dr. Pericot is scrupulously fair in his statement of controversial issues; and in dealing with chronology, that most controversial of archaeological issues, while presenting his own scheme, he includes for comparison those of Bosch Gimpera, Santa Olalla and Martín Almagro. These four schemes diverge most, of course, in their treatment of that period of prehistory from the appearance of the first Neolithic peasant villagers to the arrival of the Celts. Pericot, while not agreeing with the long chronology of Bosch Gimpera, allows an early Neolithic of before 3000 B.C., dates the first Iberian megaliths to before 2300 B.C. and gives the five hundred years from 1700 to 1200 B.C. for the duration of the El Algar culture. It seems to me that Pericot has not gone far enough in reacting from the inflated chronology which Bosch Gimpera has canvassed for over twenty years; Almagro and Santa Olalla are probably nearer the truth. It is unlikely that Los Millares dates before 1900 or 1800 B.C., or the Algaric Bronze culture much before 1500 to 1400 B.C. And, unless we propose to abandon the current typology of Iberian megaliths made popular by Fleure, Peake and Forde, and revert to the older schemes of Wilke and Leeds, the *floruit* of the Iberian megaliths is surely the second, third and fourth quarters of the second millennium B.C. But this is a point of detailed interpretation and there is no agreement on the relative and absolute chronology of Chalcolithic Iberia. Pericot makes this clear. His well produced book, with its thirty-two plates and numerous text figures and distribution maps, is indispensable to all students of western Europe in pre-Roman times.

GLYN E. DANIEL

**The Prehistoric Chamber Tombs of England and Wales.** By Glyn E. Daniel. C.U.P., 1950. Pp. xiv, 256, 16 plates, 33 text figs. Price £1 11s. 6d.

**191** An increasing need in the study of European prehistoric archaeology is the production of specialist books which not only set forth discussion and interpretation, but supply the detailed evidence on which the synthesis is built up. Grahame Clark's *Mesolithic Settlement of Northern Europe* (1936) was a supreme exemplar, and

now from another Cambridge scholar comes a work which will be indispensable to British prehistorians, and of the greatest value to all concerned with western European archaeology.

In this book, Dr. Daniel is mainly concerned with the direct presentation of factual information on the chambered tombs of England and Wales, and, it should be added, of the Isle of Man and the Scillys. The discussion of the overseas prototypes of the tombs, where they exist, is left to the final chapter, and dealt with in a restrained manner. One must await the results of Dr. Daniel's subsequent researches abroad before justice can be done to this matter. Dr. Daniel makes it clear in the first chapter, however, how he thinks chambered tombs in Western Europe, whether built of megalithic slabs or otherwise, should be classified. This is a well-known controversial point. I am in substantial agreement with Dr. Daniel's views, but neither of us would wish to propound a solution which is meant to be more than a basis for discussion and future research. The rapid development of archaeological technique demanding fresh appraisals of existing evidence only emphasizes the limited and transient nature of contemporary interpretation which is, nevertheless, essential as a debating ground for further work.

The book is divided into two parts, Descriptive Text and Inventory. The latter provides a concise description and bibliography of each one of some 215 tombs. The former contains an orderly series of chapters describing the distribution and environment of the tombs, their construction, classification, uses and contents. Of particular value to comparative studies is the information collected in Chapter V, especially in relation to the forms of entombment, the duration of the use of the tombs, and the evidence for ritual. The anatomical evidence gained from surviving skeletal material is most usefully summarized. It is surprising to learn that as many as 550 individuals are represented in the material recovered from these chambered tombs when it is considered to what ravages these conspicuous monuments have been exposed.

The book is excellently illustrated, and the text figures include seven maps.

T. G. E. POWELL

**Ahnengrab und Rechtsstein.** By John Meier. Deutsche Akad. der Wissenschaften zu Berlin, Veröfentl. der Kommission für Volkskunde, Vol. I. Berlin (Akademie Verlag), 1950. Pp. 159, 35 illus.

**192** In the form of a trilogy Professor Meier has arranged the evidence for his statement that, among the Indo-Germanic peoples, sanctuaries, seats of judgment and ancestral graves are identical. Though in point of time the latter have, of course, priority, the author believes that cult and law (which were primarily also identical) were immediately associated with the grave site.

This second volume (*Ahnengrab und Brautstein* was reviewed in *Folk-Lore*, Vol. LXI (1950), pp. 214f.) is devoted to the relationship



between the ancestral grave and the 'stone of judgment.' On account of the war and its aftermath the author had little access to the English literature of the last 12 years and often relied on Hadrian Allcroft's writings, which in view of the progress made by archaeology since their publication should be critically revised.

Instances from various parts of the British Isles could easily be added to the chapters on Inauguration Stones and Marker Crosses. Archaeologists will be interested in the pages dealing with 'the Perron' (pp. 62ff.), with 'Menhir und Grabpfahl' and 'heilige Furchen' (pp. 38ff.) and the numerous references to blue stones of judgment (pp. 103ff.). 'Primitive Folkmoths'—to use Sir Laurence Gomme's term—might be searched for ancient burials, because, as Professor Meier points out, with the diminishing ancestor-worship the grave itself was often forgotten, whilst the memory of the spectacular judicial proceedings which had taken place at its site still survived and has been handed down to us in various names alluding to seats of judgment. Folklorists will profit from the passages about the various uses of hazel rods (pp. 41ff.), the shoe-offerings (pp. 141ff.) and dances around the graveside (pp. 137ff.).

The author begins by frankly admitting that he has deliberately avoided the discussion of controversial subjects in order to make his notes accessible to future scholars. This attitude, which has repeatedly been recommended in this country, will be especially warmly welcomed by English readers. In conclusion Professor Meier expresses the wish that the study of folklore, which in his opinion holds great promise, should spread further afield and gain more depth. The third volume, on the ancestral grave and the sanctuary, which we hope to see in print soon, will certainly not fail to enhance the prestige of this special field of study.

ELLEN ETTLINGER

#### Préhistoire du Valais des Origines aux Temps Mérovingiens.

By Marc-R. Sauter. Sion, 1950. Pp. 165.

193

M. Sauter's book is in two parts, the first a summary in five chapters of the results of archaeological research in the Valais from the Neolithic to the Dark Ages, including an account of the author's important excavations at Barmuz sur Collombey. The second and longer part is an inventory (begun by M. Viollier) of all known finds from the Canton with their reference on the 'Atlas topographique' and the 'Carte Nationale Suisse,' their present whereabouts, and bibliography. There is also a general bibliography.

The illustrations include four distribution maps. Canton Valais possesses three major alpine passes and amongst points of wider interest the question of alpine traffic is discussed. The distribution of Neolithic finds, including the ambiguous Chamblandes-Glis

culture (some formerly unpublished material is now illustrated), taken with those of Val d'Aosta, suggests some use of the Grand St.-Bernard already though we are justly warned against allowing this complex too high a date. The Bronze Age map gives more evidence for the Grand St.-Bernard, while the finds from Collombey-Barmuz with those of Crestaulta in the Grisons suggest use of the Furka in the context of Vogt's Rhône-Straubing-Kisapostag group. There is no new evidence for the problem of the contemporary source of raw metals, local or East Alpine. In the Late Bronze Age Valais appears to have been a backwater. The next period saw increased use of the Grand St.-Bernard, the Furka and perhaps the Simplon. The danger of the argument *ex silentio* perhaps appears in the historically documented Roman traffic over the Furka, when Valais was administered from Rhetia, for which a few coins on the neighbouring Col de Nufenen are the only archaeological evidence.

The agricultural pattern, and probably the population itself, remained almost unaltered from the Neolithic till the nineteenth century.

This excellent study provides in convenient form the raw material for the specialist and a clear introduction and summary for other readers also.

N. K. SANDARS

#### Schweizer Masken und Maskenbräuche. By Karl Meuli. Zürich (Atlantis), 1943. Pp. 163, 60 plates

194 Although the excellent illustrations represent none but Swiss masks, the text is a profound study of the origin and meaning of masks in different parts of the world. Even in Switzerland, where masks are still made and worn, written records are scanty. They consist mainly of prohibitions issued by ecclesiastical and secular authorities and merely touch the surface of the problem. Its deeper strata could, however, be approached by examining the various occasions on which masks were worn and the behaviour of the mummers. The author's argument that mask and Totengeist were originally identical seems absolutely conclusive, though it must be admitted that no explanation is forthcoming for comic and grotesque masks.

It is regrettable that no British specimens have been included. To mention only two instances: the Welsh Mari Lwyd (discussed in MAN, 1944, 74), which resembles in an extraordinary way the *Rosgrind* from the Zürcher Oberland (see illustration facing p. 73), would have been a further corroboration of Professor Meuli's theories—whereas the Padstow Hobbyhorse, just as sinister and strange and yet so different on account of its maritime character, might have induced the author to investigate the influence of geographical environment as well.

ELLEN ETTLINGER

#### OCEANIA

#### The Coming of the Maori. By Te Rangi Hiroa (Sir Peter Buck).

Wellington (Maori Purposes Fund Board: Whitcombe & Tombs), 1949. Pp. 548. Price £1 10s.

195

This magnificently illustrated book is part of the fruits of the life-long research of one who has done as much as any man in Polynesian studies. Not only has Sir Peter Buck carried out a great deal of valuable investigation in New Zealand on physical anthropology, technology and other topics; he has also done first-hand studies in the Cook Islands, Samoa and other Polynesian groups. The result is that no one today can match his mastery of Polynesian ethnology.

This book was intended originally as a revision of several lectures he gave in New Zealand in 1925 under the auspices of the Cawthron Institute. The monograph then published has now been completely rewritten and immensely enlarged. The result is a book which gives an elaborate reconstruction of most aspects of ancient Maori culture. It first examines evidence, both from Maori traditions and from archaeology and comparative data, on the coming of the Maori to New Zealand. Sections are also given on the physical anthropology and the linguistics of the Maori people. Thirteen chapters are then devoted to a review of Maori material culture, a subject on which Sir Peter is the acknowledged expert. Further sections of the book examine some of the basic principles of Maori social organization and the Maori religious system.

The work is clearly destined to be a standard treatise on the Maori. Not only does it provide a wealth of detail in a form not before assembled, but it also gives a great deal of critical appraisal of material previously accepted and cited by many authors. His view, for example, that an exaggerated, erroneous value has been placed on the plank canoe as a culture criterion is worthy of respect. It rests essentially on consideration of such canoe-construction as being guided by limitation of supply of trees with large trunks. His observations on the traditional material recorded by Te Matorohanga and others are very pertinent. For instance, he criticizes *korowa* as a term for outrigger as having been coined by these old men to describe a structure of which they had heard but of which no details had been preserved. He also rejects lo as a pre-dispersal central Polynesian high-god, and reduces him to a local East Coast Maori development. Some extremely interesting discussion on culture change is given both incidentally and in a special section. Illuminating data are given about changes in the function of the *tiki* neck ornament, which is regarded now not as a fructifying symbol for women but as a good-luck talisman for men—by American soldiers during the last war. The infusion of critical analysis of the Maori material with constant reference to comparative Polynesian data reveals the author's great knowledge.

The book has a few obvious defects. Most marked is the inadequate treatment by modern standards of social structure and social



organization. A chapter on the family and the tribe ignores contemporary analyses of lineage structure in their relevance to Polynesian material. Sir Peter uses the term lineage to mean simply a line of ancestors. This dictionary usage, though it has now passed out of the anthropologist's vocabulary, could be justified and may be even preferable to the use of the term for a group of persons—a product of the line but not the line itself. But from a contemporary work of this kind one might have expected some mention of contemporary use of terminology. Linked with its omission is a lack of data on the constitution of the larger kin units of Maori society. There is discussion about land tenure but no very close examination of how land rights are delimited in kin terms. In particular, there does not appear to be any reference to the relative rights of men and of women, nor to the extent to which rights through women could and can be maintained through certain conditions of residence. The whole problem of kin-group membership is in fact left out of account. More discussion on social status and social stratification would have been welcome also.

But one should not cavil too much at a book of this kind which gives such a valuable synthesis of so many different kinds of materials and enables the reader to see far more fully than ever before the real significance of Maori culture as a specialized Polynesian product.

RAYMOND FIRTH

**The Moa-Hunter Period of Maori Culture.** By Roger Duff. *Canterbury Mus. Bull. No. 1. Wellington (Dept. of Internal Affairs), 1950. Pp. 405, 38 plates, 60 figs.*

**196** In his foreword Dr. Skinner points out that 'a good many archaeological papers have been published in New Zealand but this is the first archaeological book.' It is to be hoped that it will be followed by others, and that they will be of the same high standard.

The core of the book is a detailed account of the excavations carried out by Mr. Duff for the Canterbury Museum (of which he is now Director) at the Wairau Bar site, at the north-east extremity of the South Island. A number of Moa-hunter camp sites had been investigated previously; a few thoroughly and scientifically, especially by Skinner and his disciples, among whom Teviotdale was outstanding, but the majority by curio-hunters or well meaning enthusiasts who destroyed the evidence for their more competent successors. Even the objects found at some sites can no longer be traced. Some such damage was done at Wairau before Duff arrived on the scene, but much remained, and to this he applied the technique of modern excavation and his wide knowledge not only of Moa-hunter, Maori and Moriori material culture, but of Polynesian ethnography as a whole. The importance of the site lies in the fact that here, as well as the midden remains which are relatively common in the South Island and known from a number of sites in the North Island, there were a large number of burials with associated grave goods, consisting mainly of adze blades and personal ornaments. There is satisfactory evidence, including pierced moa-egg water vessels, that these burials are of the Moa-hunter period. New Zealand sites are shallow—depths are usually measured in inches rather than in feet—and at no site does a Maori level overlay a Moa-hunter one. The normal evidence of stratigraphy thus being not available, the importance of Wairau is clear.

The cultural origin of the site thus established, Duff was able to proceed to a classification of adze blades, fish hooks and lures, personal ornaments and other artifacts, not only of the Moa-hunters but for the whole of Polynesia. He demonstrates that certain types are widely distributed, not only in marginal areas of New Zealand—the South Island and the Auckland Peninsula—but also in other parts of Polynesia marginal to the Tahitian group; in Tahiti itself these forms have mostly not been found. Thus archaeology supports tradition in regarding Tahiti as the great dispersal centre for Eastern Polynesia. This classification is an important contribution to Polynesian ethnography. It is not of course entirely new—the author acknowledges his debt particularly to Skinner—but as a result of the Wairau discoveries it is on a sounder basis than ever before; and though it may have to be modified it seems unlikely that it will be superseded. The theoretical implications of marginal distribution are admirably discussed. The possibility of diffusion from one marginal area to another is given due weight (for instance between

Mangaia and Rurutu, and the spread of Maori culture from the North Island), but the weight of evidence pointing to Tahiti is incontestable.

In the introductory section the place of Moa-hunter culture in Polynesian ethnography is examined and the history of human settlement in New Zealand traced. The author concludes that the Moa-hunter culture was that of the Pre-Fleet tribes, and that in the North Island the moa was extinct before the arrival of the Fleet (c. 1350), though it may have survived much later in the South Island. It is possible that South Island culture was not truly 'Maori' in the time of Tasman, perhaps not in that of Cook, and that Moa-hunter elements survived here to the period of intensive European settlement. The Moriori of the Chatham Islands are regarded as descended from a Moa-hunter (pre-Fleet) settlement, with an early intrusion of post-Fleet elements. Here the extinct swan, also important in Moa-hunter economy, took the place of the moa. Duff does not consider that the theory of a Melanesian element in New Zealand—the Maruwi of Maori tradition—is supported by the evidence.

The conclusions stated in the introduction are supported not only by the extensive typological section but by an interesting chapter on 'The Evidence of Tradition,' in which the Maori accounts are examined and evaluated. There is also a series of extracts from published traditions. A survey of the course of Moa-hunter research brings out the unhappy effect of Haast's belief that the Moa-hunters were 'autochthones,' a people with a palaeolithic culture who inhabited the country at a remote period; this produced a reaction in which research was directed to showing that there was no distinctive difference between the Moa-hunter and Maori cultures. This section is valuable in that it surveys and puts in proportion the work of many of the author's predecessors in this field, much of it published in journals not easily accessible in this country. A similar stock-taking for Polynesia as a whole would be very useful.

Owing to lack of funds, the distances involved, and the dislike of the people for disturbance of their ancient sites, little excavation has been done in the rest of Polynesia. That little (mostly published in the *Bulletins* of the B.P. Bishop Museum) has mainly been concentrated on *marae* and dwelling sites. Perhaps the most interesting in its bearing on Duff's work is that of Emory on Nihoa and Necker Islands (*B. P. Bishop Mus. Bull. No. 53*), which suggests that the early history of these islands of the Hawaiian group may have been similar to that of the Chathams. Intensive work in the Society Islands might produce particularly interesting results in the form of adze types, superseded there but found in the marginal groups.

There are a few misprints, mostly not of importance; but a more serious defect is the large number of errors in references to plates and figures. For example figs. 28 and 29, given in the list of line drawings as on pages 132 and 133, are in fact on pages 136 and 137; on p. 20 there is a reference to a 'frontispiece diagram,' which is fig. 1 on p. 25; on p. 129 the reference to fig. 56 should apparently be to fig. 58; on p. 263 the reference to Edge-Partington's *Pacific Album*, 3rd series, Pl. 71, should read 'Pl. 171'; the list could be extended. The plates and figures are excellent, as are the bibliography and appendices. A map of New Zealand showing all the places mentioned might be a useful addition, especially for readers outside New Zealand.

B. A. L. CRANSTONE

**The Technology of a Modern Stone Age People in New Guinea.** By Beatrice Blackwood. *Pitt Rivers Museum, Univ. of Oxford, Occ. Pap. on Technology No. 3. O.U.P., 1950. Pp. 60, 16 plates, 53 text figs., 2 maps.*

**197** The people whose material culture is dealt with in this admirable treatise are mainly the Nauti and the Ekuti (known as the Kuku-kuku) who live about 5,000 feet up in the Ekuti Ranges in north-east New Guinea. The author lived in their villages for nine months during 1936-1937, and obviously made the best of use of her time in studying, recording and photographing the implements and processes of their simple arts and crafts. The account of the habitations, villages and gardens is condensed, and the reader is referred for more detail to the *Geographical Journal* for July, 1939. Of tools the stone-bladed adze, with shouldered haft, receives exhaustive



treatment in the text, in the photographs, and in line drawings. The processes of shaping the stone and the haft, and of fitting the two together, are represented with a clearness of detail that could scarcely be excelled. The same praise may be given to the describing and picturing of the stone barkcloth-beater, and of the processes of making the cloth; and also to the shaping, perforating and fitting of the stone club head to its shaft. Bows and arrows require less detailed treatment; incidentally, it is curious that the spear is not in use. It may also be noted that although flint is not worked, sharp-edged pieces are picked up and used as scrapers, etc. Bone awls, wooden clubs and knives, boars' tusks for planes, bamboo knives and pipes, string and netting, clothing, and personal ornaments in some

variety constitute the greater number of the other artifacts that are described. To these must be added the details and method of use of the fire-making appliance—the sawing-thong and hearth. Fire is used to help in the felling of large trees, the adze cutting away the charred wood.

Technologists will welcome further Occasional Papers from the same source as this one and its predecessors, whose high standard does justice to an invaluable museum. It might even be suggested that a republication in this series of one or more of the late Henry Balfour's technological papers would serve as a fitting tribute to one with whose name the growth and development of the museum will always be associated.

H. S. HARRISON

## CORRESPONDENCE

### A Hitherto Unknown and Undescribed Temple at Tikal, Guatemala

**198** SIR,—The purpose of this report, which is incomplete, cursory and lacking in detail, is solely to indicate the presence of a temple in the Tikal area, which to the best of my knowledge has been hitherto unknown and undescribed. If present indications are verified, it may prove to be the most important temple for research purposes in the whole Peten area, for it is the only one observed or described as having the exterior walls on the south, east and north covered with hieroglyphic inscriptions.

It has been my privilege during the month of April, 1951, to spend considerable time at the ruins of Uaxactun and Tikal, under the efficient and capable direction of the official guide to these ruins, Sr. Antonio Ortiz Contreras, who has spent more than four years in this work. It is he who took me to this particular temple, it having been reported to him only two weeks previously by three *chileros*, and this was his third trip, the other two being purely exploratory. He has consulted his maps, and I have very carefully consulted the Report on Tikal by Teobert Maler, Vol. V, No. 1, for the Peabody Museum of American Archaeology, and also the Report of the 1909-1910 Peabody Museum Expedition by Dr. Alfred M. Tozzer, Vol. V, No. 2, and neither of us has found this temple noted.

I have not been able to consult the report to the Royal Geographical Society, Vol. V, pp. 185-204, by Sir Alfred P. Maudslay, in 1883, but feel reasonably assured if he had mentioned this, those who followed him would have indicated its presence.

In the brief description following, all measurements are approximate. Time was very limited, for it took nearly one and a half hours to reach the site, starting immediately after 1 p.m. from the army base at Tikal. The late afternoon light and dense forest growth precluded taking photographs. No orientation of the temple was possible at this time.

The temple is situated approximately four miles south-east from the centre of Tikal. Unless the whole area traversed in reaching the site is one whole artificial platform, of which there is no indication, the pyramid is constructed directly upon the ground, thus differing from the central constructions at Tikal. The base of the pyramid is approximately 150 feet by 150 feet, and the height of the pyramid plus temple is approximately 100 feet. The façade faces directly west. It is completely isolated from all other buildings, temples, tombs or palaces, unless further exploration in this area reveals their presence. As mentioned above, the south, east and north walls are covered with a stucco veneer of hieroglyphic inscriptions about 1 foot square, in high relief. These are in an excellent state of preservation compared with the other ruins and it seems to me imperative that a complete survey be made as soon as possible on this temple before the usual vandalism occurs. The only indication of any human having visited here is the scratching on an interior wall of the names of the three *chileros* who first came upon it; there are no other marks, and as yet, no indication of vandalism. The excellent state of preservation would seem to indicate further either that this temple was constructed with greater care and strength, or that it was built at a much later date than central Tikal.

The temple has one big room running north to south, with three doors opening to the west, of which only the one door on the northern end can still be seen plainly, for the front wall is caved in. The room's area is approximately 30 feet by 6 feet, and it is 12 feet from the floor to the top of the Mayan arch. In the centre back of this big room an arch communicates with another, smaller room. In what would correspond to the second floor of the temple, there is still another room, slightly smaller than the one on the first floor, and partitioned by two walls. The third section is completely sealed, except for an opening on the northern corner of the temple, which tempts one to assume that it was used for solitary confinement, either for meditation or death. In the roof-comb there is evidence of another small room, suggestive of the vents mentioned by Dr. Tozzer to conserve material and lessen the weight of the superstructure on the base.

A good view of the other five great temples was obtained from the top, but this temple is not in turn seen from their tops; the jungle growth is so heavy and the trees so high that it is totally obscured. To reach it, after the long walk on a narrow trail, one must break directly through the jungle growth and it is not observed until one is directly on the site.

On the north side were noticed two unusual hieroglyphs having a bas-relief star almost directly in the centre.

No stelae and no altars were observed, but this is not to say there are none present. As we had to return to the base before dark, time at the site was extremely limited, and there was no further opportunity to look for indications of these.

It is to be hoped a competent archaeologist may make a full and detailed report on this temple and its environs before the area is opened to tourists, who will readily avail themselves of air travel now that the Government of Guatemala has an airstrip under construction at Tikal.

J. AGNES McDONALD

c/o The Canadian Embassy, Mexico City

**Social Anthropology: Past and Present.** Cf. MAN, 1950, 198, 254 and 271, and 1951, 33-35, 62, 78, 120 and 150

**199** SIR,—In this lively correspondence the pursuit of the Snark—or could it be a Boojum?—appears to have got mixed up with a trawl of good red herring, laid as live bait perhaps. Professor Evans-Pritchard must surely be more embarrassed by the arguments of his supporters than by those of the nominal opposition? Or is he merely enjoying the jest? His artful suggestion that his views conflict with 'the functional theory dominant in English anthropology today,' and hence with the opinions of 'most of my anthropological colleagues in England,' has evidently given the impression that his next book may be entitled *The Children of the Moon* or *The Megalithic Culture in Nova Scotia*. As these irrelevancies are liable to prove confusing to readers of MAN who are not also members of the Association of Social Anthropologists, I feel that it should be pointed out that 'the functional theory,' as Professor Evans-Pritchard outlines it, is not taught as orthodox doctrine in any English academic institution, and further that it simply is not true that Malinowski asserted that 'even when the history of a society is recorded it is irrelevant to a functional study of it.' On the



contrary, one of Malinowski's last comments on this topic was the following (in *Africa*, Vol. XII, p. 42):

'A comparative study of city politics in ancient Greece and Renaissance Italy; theories of revolutionary movements, of dictatorships, of social stratification, property, and the role of war and conquest in human development, can be and have been legitimately built up from reconstructed historical facts. Thus history, which thrives on sociological and cultural generalizations—for without these it becomes mere guesswork—yields again laws of social or cultural science. To oppose history and science is futile. To neglect either of them makes any humanistic pursuit incomplete.'

This substantially is Professor Evans-Pritchard's own position.

Since Malinowski's death anthropological theory in England has undergone very substantial development, largely as a direct response to Professor Evans-Pritchard's own writing and teaching. It seems both paradoxical and regrettable that he should now see himself as a reactionary and out of line with the direction of normally expected scientific synthesis.

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E. R. LEACH

**Race.** Cf. MAN, 1950, 220 and 1951, 28-32, 93-96, 101, 122, 151 and 180

**200** SIR.—In writing about 'race,' in discussing the subject of 'race,' and especially while engaged in arguments about 'race,' it is well to remember two facts. 'Race,' to the physical anthropologist, is a biological concept. And the concept of 'race,' like other scientific concepts, is not static, but is in a state of flux.

The concept of 'race' changes with time. It expands, to accommodate new findings and changes in other concepts, and it contracts, eliminating ideas that need to be discarded. The concept of 'race' held by physical anthropologists today differs in certain respects from that held only a few years ago. Therefore, when the subject of 'race' is under discussion, it is important that the present concept of 'race' and not some previous concept be considered.

In reviewing many of the discussions on the subject, it is obvious that older ideas of 'race,' including some ideas long discarded by physical anthropologists and writings of anthropologists now long dead, are still cited for the purpose of refuting them. This is a queer sport. It suggests that some of the participants are either unaware of the present status of the concept of 'race,' or that they are deliberately beating a dead dog.

The fact that the concept of 'race' has changed, and the meaning of the term 'race' has changed, bears on the search for the origin of the term and its 'original' meaning. Granted that this subject is of considerable historical interest, it is still unlikely that the original meaning of the term will coincide with its present use, nor is there any need to make the present usage conform to the original meaning. The present concept of the atom is in no way influenced by the fact that the word 'atom' originally meant that which cannot be split. The present concept of 'race' is similarly uninfluenced by earlier uses of the term 'race.'

The biological concept of race changes to accommodate new lines of thought. Similarly the definition of 'race' is by no means fixed; rather it changes under the influence of current thinking. Therefore it is not possible to establish one definition of 'race' that will hold for all time, unless we are willing to exclude new findings and all new changes in thought. But it is possible to provide definitions of 'race' that are in accordance with current ideas, and this physical anthropologists have done. Current definitions of 'race' emphasize the fact that a race is a population that is at least temporarily distinguishable from other populations by virtue of a distinctive pattern of anatomical and physiological traits, as a result of relative isolation and the effects of natural selection.'

In classifying mankind, physical anthropologists do not pretend that they have set up a taxonomy that will always be useful. The races that we distinguish today were not so distinguishable in the Mesolithic, and in the future new races will have to be distinguished. What is important is that the classification should coincide with the observed facts, and that the number of categories or races be neither too large for convenience nor so small that markedly divergent populations are included under the same label.

Interest in human taxonomy does not imply that taxonomy itself or a taxonomic system is the ultimate goal of physical anthropologists. Racial differences are not important in themselves, but become important when human evolution (in which race-formation is one step) is under consideration. What environmental factors have brought about particular racial differences? To what extent are these differences due to natural selection? To what extent are similarities due to genetic relationships or to convergent evolution? To what extent do barriers to reproduction, natural or man-made, speed race-formation? In order to answer these questions, either in a general way or for specific examples, it is necessary to know what the differences and similarities are. Hence the need for taxonomy.

Now so far we have considered the biological concept of race and the definition of race as used by physical anthropologists. However, the term 'race' is used differently by many sociologists, in popular writing, in psychology, and by politicians. This is the natural consequence of attaching a somewhat specialized meaning to a word in the public domain. The same situation prevails when words like 'culture' or 'personality' are taken from the popular vocabulary and assigned more restricted meanings. The specialist and the layman often use the same word in different ways.

It is here that the suggestion has been made that the use of the term 'race' by anthropologists favours the continuation of social and economic discrimination against particular groups, discrimination based on irrational prejudices, discrimination often ineptly termed 'race prejudice.' And the further suggestion has been made (both by individuals who dismiss the concept of race as 'a thing of the mind,' and by scholars who admit the reality of race, but are concerned with the unscientific doctrine of racism) that the term 'race' should be abandoned, or replaced by some innocuous substitute.

If excising the word 'race' from the dictionary would really accomplish a humanitarian purpose, one could hardly object to the sacrifice. No word is worth human blood. But from what is known about people with prejudice (whether it is 'race' prejudice or 'colour' prejudice or 'religious' prejudice) such irrational feelings do not exist singly. The prejudiced person possesses a constellation of dislikes, fears and hatreds, and lexical surgery is hardly a cure for the disease. It is highly unlikely that abandoning the term 'race' would serve any useful purpose, and while such terms as 'ethnic group' or 'ethny' have a legitimate use as labels for populations or groups that are neither races nor sub-races, they should not be substituted for the term 'race' in scientific writing, when 'race' is what is meant.

It is best for anthropologists to understand the biological usage of the term 'race' and to be familiar with the concept as currently held. If it is deemed necessary to criticize older ideas on 'race' and tenets no longer held by physical anthropologists, this fact should be made clear. The concept of 'race' is a changing concept; the definition of 'race' and the 'races' that are distinguished are not unalterable. Finally it should be clear that group prejudices will not be eliminated simply by denying the existence of 'race,' deriding the study of 'race' or forbidding the use of the word 'race.'

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#### Note

<sup>1</sup> See S. L. Washburn, 'Thinking about Race,' *Science Education*, Vol. XXVIII (1944), pp. 65-76; C. S. Coon, S. M. Garn and J. B. Birdsall, *Races: A Study of the Problems of Race Formation in Man*, Springfield (C. C. Thomas), 1950; E. A. Hooton, *Up from the Ape*, New York (Macmillan), 1946; and T. H. Dobzhansky, 'Nature and Origins of Races,' in *Encyclopedia Americana*, Chicago (Americana Corp.), 1950.

#### 'Races' and 'Natives'

**201** SIR.—The controversy in MAN over the concept of 'race' has now brought into issue the use of another term—'native'—to which Dr. Trevor refers in his reply to Mr. Dover (MAN, 1951, 96), though he does not go beyond saying 'anthropologists study both races and natives.' In the same issue of MAN there is a review (1951, 88) of Mr. Grenfell Price's



book *White Settlers and Native Peoples* by Lord Raglan, who surpasses Mr. Price by combining the two terms 'native races.' But the greatest effort in confusion was left to Dr. Piddington in a chapter heading of his book *An Introduction to Social Anthropology*, Vol. I (reviewed in MAN, 1951, 85), which reads 'The Native and Ourselves.' Who are 'Ourselves'? And who is 'The Native'? Any attempt to give a consistent meaning to the term 'native' in all the above contexts will reveal an undesirable uncertainty surrounding the concept.

There is no reason why the difficulty over the word 'race' should be extended to 'native,' which, used scientifically, ought simply to denote any person in any place who has not been subjected to the influence of any other culture besides his indigenous one. With this connotation, every country in the world has its own 'natives' (though the proportion in one country may be greater than in another) and the use of phrases such as 'white settlers and native peoples,' 'native races,' etc., becomes unsatisfactory, if not entirely meaningless.

O. BATEYE

London

## Note

Objections to the paternalistic, and still more to the racialistic, overtones which have sometimes been associated with the use of words like 'native' enjoy widespread sympathy nowadays; and there is a growing tendency to use them with care. Yet words cannot be more than symptoms of a disease; changing the words will not cure the disease, and conversely if the disease be abated the words can do no more harm; the English and all other languages are full of dead metaphors and other words which have become harmless in this way, and have been suffered to survive because the purposeful coining of substitute words is both difficult and distasteful to all but specialists in jargon. Lexical surgery (in Dr. Garn's excellent phrase) may have strange effects: our correspondent will be aware that in the Nigerian Government service European officials are officially styled 'expatriates,' and African officials—*horrendum dictu*—'non-expatriates.' Clearly this means neither more nor less than 'native'; and when a government or a people is willing to make such a change, it might be thought that the change is already unnecessary.

It is true that all countries have their 'natives'; but only the natives of plural societies are commonly so described in practice, the correlative being some such word as 'settlers.' Is this any more objectionable than the verbal distinction between the District Officer and the Native Authority? The principle suggested by our correspondent's letter seems to be that the noun 'native' should be used not absolutely but relatively, that a man is not just a native, but a native of his country. But no one but the very naïve would now speak of having met a native (meaning, say, an African) in the streets of London.—ED.

## The Shilluk King-Killing

**202** SIR,—My attention has been called to some critical remarks about my Frazer Lecture of 1948, *The Divine Kingship of the Shilluk of the Nilotic Sudan*, made by Dr. Lagercrantz in his *Contribution to the Ethnography of Africa* (1950). Among these remarks he says (on p. 428), speaking about myself: 'As regards the Shilluk, he asserts that regicide "was recorded as early as 1905"; it has, however, been known ever since 1882 in easily available literature.' The passage in my lecture to which Dr. Lagercrantz refers is: 'It was recorded as early as 1905 that it was a Shilluk custom to kill their kings and much information about the Shilluk kingship in general has since been collected' (p. 1).

I wrote to Dr. Lagercrantz to ask him whether he would be kind enough to let me have the references to killing the Shilluk king which I had missed and he courteously referred me to G. Beltrame's important book *Il Fiume Bianco e i Dinka* (Verona, 1881) and to a paper, 'Reise des Missionars G. Beltrame an den Weissen Nil und zu den Denkastämmen,' by G. Reymond in Vol. IV of the *Jahresbericht der Geographischen Gesellschaft in Bern*, 1881-1882 (Bern, 1882, pp. 101-119). The paper is a report on the book.

The relevant passage in Beltrame's book is on page 78:

'Il regno dei Shluk non è ereditario, come scrisse qualche viaggiatore. Quando io visitavo per la terza volta questo regno e

mi trovavo a Dénab il 7 dicembre 1859, il Re, di nome Mievddök, era già morto sin dal febbraio dello stesso anno, e nelle sue ultime agonie veniva finito con tre colpi di lancia da uno dei suoi parenti più stretti, perchè disdice ad un Monarca sì grande il morire, come essi ripetono, d'una morte troppo comune. Egli sen giaceva ancora insepoltito, ben chiuso in una capanna, perchè non era stato eletto il successore, che si diceva dover essere il figlio di un suo fratello chiamato Ghèn. "La scelta però dipende dal voto del popolo, e appena il successore sarà stabilito in carica, il defunto Monarca verrà seppellito sotto un tamarindo presso la residenza reale"; così diceva a' miei barcaioli un vecchio Shluk, che mostravasi dolente di una nuova elezione, e che meglio era, andava ripetendo, di vivere senza Re.'

The relevant passage in the *Jahresbericht* is on page 108. It is merely a summary of the paragraph in Beltrame's book which I have quoted above, so I give no more from it than what are in this context the most important sentences:

'Das Königthum ist nicht erblich. Im Februar 1859 war der alte König gestorben. Man hatte ihm den Todeskampf durch drei Lanzenstiche erleichtert, welche ihm einer seiner nächsten Verwandten beibrachte, weil es sich für einen so grossen Monarchen nicht schicke eines ganz gewöhnlichen Todes zu sterben.'

Whether I deserve Dr. Lagercrantz's strictures I leave others to judge; but I am grateful to him for giving me the reference to this early account of the death of a Shilluk king, and I have no excuse for not being acquainted with it. I am sure that it will be of interest to others, for, as far as I am aware, it is not mentioned in any later writings on the Shilluk. It will be noted that Beltrame's statement adds yet another manner to those better known in the literature in which Shilluk kings are said to be put to death. His king Mievddök is the same as king Nyidok, the twenty-second Shilluk king in the lists given by Father Hofmayr (*Die Shilluk*, 1925, p. 42), Professor and Mrs. Seligman, following Hofmayr (*Pagan Tribes of the Nilotic Sudan*, 1932, p. 44), and Mr. Howell and Mr. Thomson (*Sudan Notes and Records*, Vol. XXVII, 1946, p. 84). The Shilluk tradition recorded by Father Hofmayr, our main authority for Shilluk traditions, gives (p. 101) a rather different account of his death and one more in accordance with what we have been told elsewhere about Shilluk king-killing. According to this tradition, King Nyidok (1845-1863), who was a kindly and well beloved man, was badly wounded in a fight between his supporters and those of a rival prince.

'Nach Shilluk-Gesetzen darf ein König nicht leiden, nicht krank sein, nicht Schwäche zeigen, da sonst das Land mitleiden würde. Als daher Nyidok in seiner Hütte krank lag, kam die Mutter des späteren Königs Kur, eine gebürtige Dinka. Sie bemühte sich anscheinend, mit einem Flügel die Fliegen von den Wunden des Königs wegzufächeln. Erst auf ein vor der Hütte ertönendes Geräusch, welches das Nahen der Oror meldete, führte die Frau mit ihrem Messingring gegen Nyidok's Schläfe einen starken Schlag. Der König soll nach dem ersten Schlag noch die Kraft gehabt haben, röchelnd zu antworten: "Ya yang—dwai ji (ich bin fertig—hole die Leute)." Die Oror kamen, und nach wenigen Augenblicken hatte der König ausgelebt.'

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E. E. EVANS-PRITCHARD

## Corbelled Huts in Africa. Cf. MAN, 1951, 82

**203** SIR,—In Mr. James Walton's article on *Corbelled Stone Huts in Southern Africa* (MAN, 1951, 82), he suggests a spread of corbelling from the Mediterranean to South Africa, but adds: 'Until some beehive huts are discovered in Ethiopia or the Sudan, this can be nothing more than a hypothesis, but the distribution of corbelling certainly follows two well defined routes focusing on the Mediterranean.' Corbelled huts in the Sudan have yet to be found, but corbelled tombs have been recorded both from the Sudan and Sinai and are still being built in Eritrea. See my article on *Beehive Graves in the North-Eastern Sudan and Sinai* (MAN, 1935, 21). These may provide the architectural link required.

Survey of Egypt

G. W. MURRAY



# MAN

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The price per copy of the monthly issues of MAN has remained at 2s.—or 1s. to Fellows of the Royal Anthropological Institute—since 1921 (though the discount for annual subscriptions was withdrawn in January, 1948). The cost of producing and posting a year's issues, on the other hand, has increased from about £550 in 1939 to about £1300 in 1951, whereas sales have risen in the same period only from about £450 to £850. Thus, considered in financial terms, MAN has required an increasing subsidy from the general funds of the Institute and is now run at a substantial loss—though the Council is well aware of the advantages accruing to the Institute in other ways from its publication.

Moreover, there is great pressure on the available space—even though a word-count shows that the redesigning of the type face and type area on the page allowed about 20 per cent. more matter to be published in 1950 than in 1939. To accommodate a larger proportion of the worthy material received for publication, the Hon. Editor and the Council are anxious to publish 16 pages every month instead of only six times in the year, and the additional cost of this increase will be about £150 a year.

It has for these reasons been decided to raise the price of each monthly copy by 6d. from January, 1952, and at the same time to introduce the regular 16-page issue (this will in fact be anticipated in the closing months of 1951). The volume of material published will then be about 40 per cent. greater than in 1939, while the full annual subscription will be 50 per cent. higher. (It has not been found possible to increase the present discount of 12s. a year to Fellows, and the increase is being applied uniformly to all subscriptions.) A greater increase has been avoided only because (i) rise in circulation has partly offset the doubling of production costs since before the war, and (ii) the still substantial monetary deficit will continue to be borne by the Institute in the hope that steadily increasing circulation will eventually make of MAN a financial, as well as a scientific, asset in the advancement of anthropology.

The new rates will therefore be:

Each issue	2s. 6d. (to Fellows 1s. 6d.)
Annual subscription	30s. (to Fellows 18s.)

Subscribers are earnestly requested to make early arrangements to alter their Banker's Orders, or otherwise to pay the increased subscription in good time. An amended form of Banker's Order will be circulated with the October issue of MAN.

W. B. FAGG

Hon. Editor













(a) *Abduction scene*



(b) *Sex-act motif*



(c) *Three-horned animal (stag?)*



(d) *Three-horned bull and man with shield*

**ROCK ENGRAVINGS AT KUPGALLU HILL, BELLARY, MADRAS**

*Photographs: Royal Anthropological Institute*



# THE ROCK ENGRAVINGS OF KUPGALLU HILL, BELLARY, MADRAS\*

by

COLONEL D. H. GORDON, D.S.O., O.B.E.

**204** Rock engravings have been discovered at a number of sites throughout the Indian sub-continent, stretching from those in the north-west, recorded by Sir Aurel Stein in Baluchistan, to those in the south-west at the Edkal Cave close to Ganipativallam in the Wynaad. It has been even more difficult to date these engravings than it has the rock paintings, their crudity in most cases making the identification of cultural indications almost impossible. The swords, shields, horse-riding and Kharoshti inscriptions of the Mandori and Gandab engravings near Attock, the swords, shields, palanquins and elephant howdahs of those at Gombaguddi Hill, Jamakhandi State, and the horse-riding at Bangalore all indicate an early historic rather than a prehistoric dating. The engravings at Kupgallu Hill near Bellary (Plate I) are even more difficult to date. The humans are all extremely crude and, with the exception of one which appears to hold a shield (Plate Id), there are no cultural indications whatsoever to date the greater bulk of this primitive art output.

Kupgallu or Peacock Hill is three miles to the north-east of Bellary, and near the recently excavated site of Sanganakallu, the engravings being on the boulders of a prominent trap dyke. A number of photographs, which are mentioned by Bruce Foote, were taken and prints sent to the Madras Museum and the Royal Anthropological Institute. What the present condition of the prints in the Madras Museum may be I do not know, but the greater part of those in the R.A.I. have faded to an extent that makes them quite worthless. Fortunately the original prints at the R.A.I. were re-photographed some years back, thus preserving the record. No pictorial record of these engravings has to my knowledge been published and the only detailed description of them that I have been able to trace is that of F. Fawcett in the *Asiatic Quarterly Review* of 1892. It seemed desirable therefore that these important engravings, which are constantly mentioned, should be published with illustrations and more up-to-date comments. They are important because they present a number of interesting and peculiar features not present in other Indian rock engravings. Also they are the only ones which can be closely associated with a number of archaeological sites.

The trap dyke on which these engravings are made runs roughly west-north-west, cutting across the north-east slopes of Kupgallu Hill. On this hill two settlements were recorded, one by Bruce Foote, on the north-west and the other by Fraser on the south-east slopes. No systematic excavations were carried out, but it appears likely from Bruce Foote's description of his finds that if properly

excavated these sites would show a succession of cultures similar to those brought to light by Mr. B. Subbarao on Sannarasamma Hill, close to Sanganakallu, about a mile to the south-west. In the immediate vicinity there are menhirs, stone circles and cinder mounds, the whole area round Bellary having been thickly populated by the stone-axe-culture people and their successors.

Bruce Foote, who briefly describes some of the paintings in his *Indian Prehistoric and Proto-historic Antiquities: Notes on their Age and Distribution*, is quoted as mentioning several obscene figures, without at that time realizing what a peculiar and interesting thing this was. Apart from men or animals with exaggerated genitals, displayed to indicate their sex, subjects which might be termed obscene, such as men and women engaged in some form of sexual intercourse, are not only rare but wholly non-existent at any of the known sites where rock engravings or paintings have been found. Except therefore at Bellary, early artists in India who left their mark on rocks and cave walls were not prompted either by a religious or a ribald urge to depict the sex act.

At Bellary however five such engravings have been recorded and having regard to the profusion of these *graffiti* as indicated by the photographs there may well be a number more. In one instance the woman has her hands on her knees and the man takes her from behind (fig. 1b), this attitude being common in erotic temple sculptures of the eleventh and twelfth centuries, particularly at Khajuraho and in Orissa. It is interesting to note that, besides these relatively recent examples, this posture appears on a sealing from Mohenjo-daro, where we see a figure subduing wild beasts, a bird, a man and woman having intercourse as described and a double-headed animal. This may be seen in Plate 116, 14 of Marshall's *Mohenjodaro and the Indus Civilization*, with a somewhat bowdlerized copy on Plate 118, 10c.

Plate Ia shows a man, whose intentions cannot very well be mistaken, gripping a woman by the hair. This scene of abduction can be paralleled in a rock engraving from Gandab, near Attock, N.W.F.P. (M. E. and D. H. Gordon, 1941, Plate 12, fig. 17) and a cave painting from Jambudwip No. 4 shelter, Pachmarhi, C.P. (Gordon, 1936, Plate 14b), but in neither of these is the sex element so explicit. A parallel is to be found in one of the Vijayanagar temples, which are completely void of obscene carvings save in one instance: high up on the inside of an otherwise practically blank enclosure wall of great height and length is a small carving of a man grasping by the hair a woman who is bent over, and approaching her sexually from behind. From its singularity and its position, one can only suggest that its

\* With Plate I and a text figure



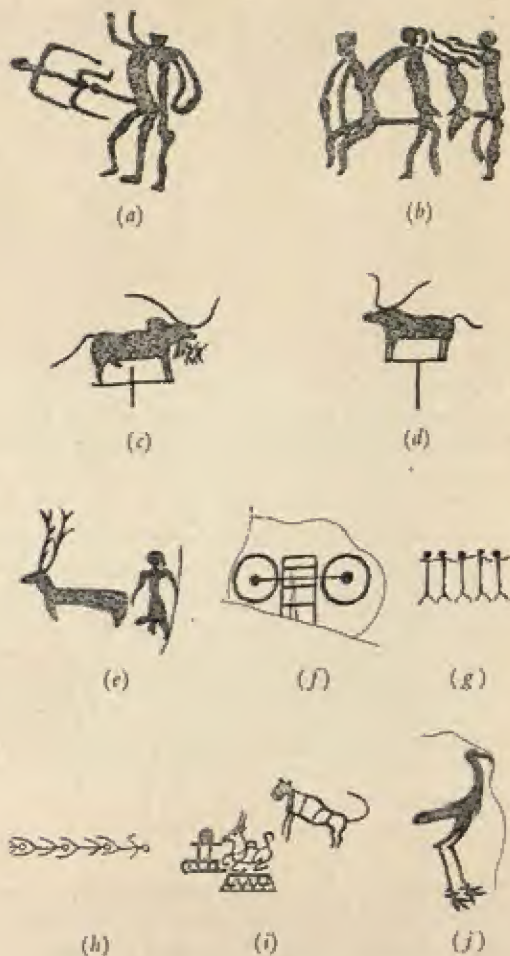


FIG. 1. ROCK ENGRAVINGS AT BELLARY

(a, b) Sex act motifs; (c, d) bull figures on supports; (e) deer and man; (f) cart; (g) dancing figures, Monte Rosa, No. 4, Pachmarhi; (h) 'stick' figures in dark red paint, Monte Rosa, No. 4, Pachmarhi; (i) lingam, Nandi bull and tiger; (j) bird (bustard?)

intention was to absorb the evil eye and also possibly the worldly passions. That any of these engravings of erotic subjects at Bellary were produced for the same reasons as prompted those present in some temples is most unlikely.

Fig. 1a and Plate 1b show two stages of the same strange artistic convention. In the first the man is approaching the woman, who is shown in a different plane of vision, as viewed from above; in the second the woman, similarly placed, has her hands on her knees and intercourse is taking place. Frobenius in his book *Kulturgeschichte Afrikas* records three instances of this uncommon convention, one (fig. 102) from Tepe Kazineh, Elam, painted on the neck of a storage jar, and two (figs. 106 and 107) rock engravings from Tel Issagen I, Fezzan. The fifth instance observable on the photographs shows a man with a very exaggerated phallus joined to a woman frontally both standing. So much for the so-called obscene figures, the intention of which is now very difficult for us to judge.

The next interesting feature is the three-horned animals, of which two are recorded (Plate 1c and d), both having

humps, though one has what appear to be tines just below the tips of the long exaggerated horns. It is arguable that the object between the horns of this example is an emblem, like the tridents between the horns of oxen on the Harappa cemetery pottery, and the similarly placed double-axes of Crete and Mykenae, but in the other example we have three definite and identical horns, which argues for a three-horned intention in both cases. I have been able to find only one other instance of a three-horned animal, for what it is worth, and this is on a painted sherd of Sialk III7 (Ghirshman, 1938, Plate LXXXII, A, 14).

Besides the animals with the three horns there are also figures of oxen apparently raised on T-shaped stands (fig. 1c, d). Whether it was the intention of the artist to depict anything of the kind it is now impossible to say, but the fact remains that there is a definite parallel between these figures and the animal standards shown on Mohenjo-daro sealings (Marshall, 1931, Plates 116, 8 and 118, 9). There is no doubt that these have every appearance of being standards of this type, though the manufacture of such objects does not tally with the material culture of any of the neighbouring settlements that have been excavated or with the technical ability of the people who occupied them.

It is interesting to note that an engraving of a cart is executed in the same way as obtains at Mandori near Attock, N.W.F.P. (M. E. and D. H. Gordon, 1941, Plate 8, fig. 7), i.e. viewed from above showing a ladder-like body frame and laterally disposed wheels, so displayed as to indicate by their roundness the fact that they are wheels. It is hard to believe that this convention for depicting a cart was not diffused from some single point of origin. (fig. 1f).

In addition to these more peculiar engravings there are large numbers of representations of men, animals and birds, mostly unrelated but in some instances possibly depicting a round-up of cattle. Two elephants stand back to back and there is at least one stag (fig. 1e). Lines of stick men dance arm in arm and also pressed closely front to back across two of the rocks, reminding me of aboriginal dances that I had seen performed by the Hos in Bihar. This continuous repetition of stick men is paralleled only by a long band of such figures painted in the shelter of Monte Rosa No. 4, Pachmarhi, where they are continuous for 18 feet (fig. 1g, h). Fawcett is quite positive that no horse is anywhere shown in these pictures, but he records men with bows and arrows, some of whom appear, to judge from one of the photographs, in a cattle-raiding scene, and mentions also a picture of two men stirring a pot with long sticks.

Fawcett was told that a well executed and nearly life-size figure with the head 'almost covered by some pitch-like substance' represented Vitlappa, 'the god who drew all the pictures.' As far as can be judged from his description the bulk of the pictures are bruising. From the photographs, taken at some distance, it is difficult to verify the exact technique, but true rock bruising, which are very scarce among the numerous rock engravings in the Attock-Campbellpur area of the Middle Indus (M. E. and D. H. Gordon, 1941, Plate 5, fig. 2), are quite unmistakable, as the rock surface has its colour changed by bruising and no pecking or even roughness is perceptible to the touch, and



it is unlikely that Fawcett would have used this term if there had been pecking of any perceptible depth.

It is clear both from the photographs and from Fawcett's report that the pictures are not of one period, but have been added to from time to time. Most conspicuous in the photographs is an engraving of a Nandi bull couchant before a Siva Lingam (fig. 11). Fawcett regards this group as 'recent,' but unless one is able to judge age by the degree of colour change, which is doubtful, it is impossible to be more exact than to suggest that it may date any time from about 700 A.D. onwards. If, as is the case with the engravings at Bangalore (Gordon, 1945), there is a distinct colour variation as the result of weathering, it should be possible to divide some of the pictures into sequence groupings. It seems likely from an examination of the photographs that the greater bulk of these pictures are of some considerable antiquity and may well be the work of the stone-age people who settled in these parts, as far as one can judge from the excavations at Brahmagiri and Sanganakallu, early in the first millennium B.C.

Before one makes any rash assertions about bull standards and Indus Valley connexions, even long-distance ones, it would be as well to verify whether the T-shaped objects supporting animals are of the same period as the animals themselves or whether they are later additions. The resemblance is an intriguing one, and the chalcolithic stone-age people's flake-blade equipment, particularly as evidenced by the material excavated at Maski and Kallur in the Raichur District of southern Hyderabad, has all

the appearance of cultural contacts with the higher chalcolithic civilization of the Indus.

That these engravings are older than any save the very earliest of the rock paintings seems to be very likely, and a provisional date of somewhere in the earlier half of the first millennium B.C. should cover the greater part of those pictures that can be associated with human figures which are not equipped with swords and shields.

These notes are unsatisfactory in that while I have seen very many rock paintings and engravings all over India, I have not seen those at Bellary; they will however serve to put this material more clearly on record until a more detailed report is forthcoming.

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## A NOTE ON TASTE-BLINDNESS IN KENYA AFRICANS AND ARABS

by

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**205** Fox in 1931 discovered that some persons cannot taste crystals of phenylthiourea (P.T.C.) and in the same year Snyder showed that the 'taste-blindness' trait is inherited as a simple Mendelian recessive. Soon afterwards, attempts were made by Lee (1934)<sup>1</sup>, the Boyds (1937)<sup>2</sup> and others to determine the frequency of occurrence of the trait in different races. The results of these investigations are recorded in Ruggles Gates's *Human Genetics* (New York, 1946); it is apparent that there are certain discrepancies which can be attributed to difficulties in applying the tests and to the use of unstandardized techniques—tasting P.T.C. crystals, papers soaked in P.T.C. and so on. As a result of the work of Setterfield, Schott and Snyder (1936)<sup>3</sup> it is now evident that the simple classification into taster and non-taster is a reflection of a bimodal distribution of taste thresholds. Recently, Harris and Kalms (1949)<sup>4</sup> have developed a technique for measuring taste thresholds for P.T.C. which includes a sorting test. Because their test is entirely objective, it is particularly well suited for use among primitive races; although it takes far longer to carry out, the findings are considerably more reliable.

The present communication embodies the results of taste tests with P.T.C. carried out on 208 Giriama, an Eastern Bantu tribe, and 63 Melinde Arabs. This investigation was part of the work undertaken while I was a member of the Oxford University Mount Kenya Expedition, 1949; it was generously assisted by grants from the Royal Anthropological Institute and the Viking Fund. Care was exercised so that, as far as possible, only racially pure and unrelated individuals between the ages of 15 and 45 were tested. The tests were carried out as recommended by Harris and Kalms, except that for convenience in the field only seven different solutions were used, having the following concentrations of P.T.C.:

1	.	.	1300.00	mgm./litre
2	.	.	325.00	" "
3	.	.	81.25	" "
4	.	.	20.31	" "
5	.	.	5.08	" "
6	.	.	1.27	" "
7	.	.	0.32	" "

The results of the tests are given as percentages in Table 1. The sex differences are insignificant and are not recorded.



TABLE I

Taste threshold	1 or >1	2	3	4	5	6	7
Giriama	2.9	0.9	0	13.5	38.5	29.0	15.2
Arabs	22.2	3.2	3.4	4.7	33.3	23.8	9.4
English	14.9	13.0	5.6	18.4	33.5	20.0	4.7

If histograms of the different thresholds are made, it is apparent that the boundary line between 'tasters' and 'non-tasters' can be drawn between groups 2 and 3 in every case. Hence the following data for non-tasters can be deduced (gene frequencies calculated on the assumption that the trait is a simple recessive):

Giriama	3.8 per cent.	Gene frequency	0.195
Arabs	25.4 per cent.	Gene frequency	0.504
English	27.9 per cent.	Gene frequency	0.5285

Lee tested 110 Kenya natives, using P.T.C. crystals, and found 8.1 per cent. non-tasters. However, in a much larger group of 805 Sudanese natives he found 4.2 per cent. non-tasters, a figure which agrees well with the incidence of non-tasters amongst the Giriama (3.8 per cent.). Barnicot (1950)<sup>6</sup> reports that there are 5 per cent. of non-tasters amongst a mixed group of African students in London. It is possible that the incidence of taste-blindness is slightly higher among the West Africans than the Eastern Bantu, but this point could be decided only by further testing.

The figure for non-tasters among the Arabs (25.4 per cent.) is based on a comparatively small sample (63 individuals). However, it corresponds to the percentage of non-tasters found by the Boyds among the inhabitants of Assiut (25.3 per cent.) and the Cairo Mohammedans (20.7 per cent.). Much higher figures were given by Parr in 1934 for taste-blindness among the Syrian Arabs (36.5 per cent.) and Armenians (32 per cent.); these should be confirmed by objective methods before they are finally accepted, but a greater incidence of taste-blindness would be expected among these peoples than in Arabs whose cultural contacts have been with Africa and who have inevitably absorbed some African blood through intermarriage with slaves.

## Notes

<sup>1</sup> B. F. Lee, 'A Genetic Analysis of Taste Deficiency in the American Negro,' *Ohio J. Sci.*, Vol. XXXIV (1934), pp. 337-342.

<sup>2</sup> W. C. and L. G. Boyd, 'Sexual and Racial Variations in Ability to Taste Phenyl-thio-carbamide, with some Data on the Inheritance,' *Ann. of Eugen.*, Vol. VIII (1937), pp. 46-51.

<sup>3</sup> W. Setterfield, R. G. Schott and L. H. Snyder, 'The Bimodality of the Threshold Curve for the Taste of Phenyl-thio-carbamide,' *Ohio J. Sci.*, Vol. XXXVI (1936), pp. 231-235.

<sup>4</sup> H. Harris and H. Kalmus, 'The Measurement of Taste Sensitivity to Phenylthiourea (P.T.C.),' *Ann. of Eugen.*, Vol. XV (1949), pp. 24-31.

<sup>5</sup> Data of Harris and Kalmus (see note 4). Other European figures which they quote are very similar.

<sup>6</sup> In a personal communication.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

### Anthropology and Education: Some Proposals for Collaboration. By Professor Margaret H. Read.

206 Summary of a communication to the Institute, 17 April, 1951

There is a justifiable belief that among all the modern influences which change the composition and character of a formerly static society there is none more potent than the effects of modern education. Anthropologists would agree that the cumulative effects of modern education can be examined in terms of the stability and cohesion of the society which they are investigating.

There is a certain veiled hostility between the anthropologist and the educationist, whether the educationist is of the same ethnic group as the anthropologist or whether he belongs to the society which the anthropologist is investigating. It is admittedly a difficult issue. It can be exaggerated but it is inherent in the different approach by each to the society he is interested in. While it is true that the friction between workers in these two fields has lessened considerably, it is mainly because the educationists have begun to see the relevance of anthropological studies to their particular work. So far, in this country at any rate, there has been little evidence of the interest of anthropologists in the process or results of modern education in formerly pre-literate societies. That is one reason why it seems that the time is ripe for some proposals for collaboration between the two groups of workers. I am going to present these proposals under four different headings. The first has been recognized by anthropologists for many years but very scantily treated by them. It is what used to be called 'Primitive Education.' The second is perhaps the result of the gradual spread

of interest among educationists in the principles and methods of social anthropology, and has reference to the curriculum in the schools and its relation to the cultural background of the people. The third is the relation of sociological studies to the promotion of education among adults, especially in certain fields such as agricultural extension and community development. The fourth is the need to analyse schools and other similar groupings as social institutions in a changing society.

These four headings present very different types of problems for the anthropologist and they are of course selective and in no sense comprehensive. I hope this selection and grouping of problems will stimulate investigation and research into a field which we have hitherto failed to recognize as an important one in social anthropology. I am assuming in this paper that we are discussing this relationship in terms of countries and societies where modern education has been introduced relatively recently. I have no doubt that parallel studies in this country and elsewhere would yield important and interesting results which might throw light on the problems which will now be considered.

As this paper has necessarily covered a wide and somewhat divergent field, it may be useful to sum up the proposals made. It should be clear that I have no doubt at all about the importance as well as the necessity of collaboration between anthropologists and educationists. We need more extensive and intensive studies of the process of socialization, both in tribal areas and in those where the cultural pattern is not within a tribal structure. Where there is demand for the inclusion of local cultural elements in the school curriculum, anthropological advice is wanted on the function of vernacular languages in the local culture; on the study of



folklore and proverbs and their function as oral tradition; on the technical and æsthetic aspects of indigenous art and music, and again on their function in the local culture; and on the many aspects of tribal and local history which are part of the oral tradition.

In the field of the education of adults, I have emphasized the contribution which anthropology can give to planning in community development; and to more effective instruction in agriculture and other rural activities because the instructors are better informed about the current practices and the values and attitudes attached to them. Finally I have suggested that anthropologists might give some attention to studying schools and similar groupings as social institutions, both at the village level and on a wider scale.

## SHORTER NOTES

### Recent Archaeological Work in Czechoslovakia. Communicated by S. E. Mann

**207** The following translations of articles which appeared in *Lidové Noviny* for 11 February and 22 February, 1951, respectively may be of interest to readers of MAN.

#### I

A large audience attended the second evening session of the Prague archaeological working party for the current year and heard about two new mesolithic discoveries. Remains of this period covering several millennia are to be found in the sand ridges along the Elbe in Bohemia. These stretch as far as the outskirts of Prague, and appear also at Slánsko, at the foot of Krušná Hora and in southern Bohemia, where the only site at present known is that of Ražice by Lake Režabinec, near Písek. Here the finds have been most rewarding.

This southern Bohemian site seems to belong to the oldest mesolithic stratum, representing a period when the palæolithic hunters were retreating northward after the post-glacial thaw had set in, and new settlers were taking their place who lived mainly by fishing and berry-gathering. On the evidence of the famous French site, this cultural stratum is known as Tardenoisian.

The Ražice site spoken of by Dr. Mazálek is interesting in many ways. The southern Bohemian terrain where these finds were made was obviously not essentially different from what it is at the present day. An expansive sheet of water like Lake Režabinec, now a national trust, must have attracted mesolithic man of some 8,000 years ago towards the arts of fishing, hunting and game-trapping.

Throughout the past year a number of pit settlements were unearthed of a kind without known parallel in this country, though similar Tardenoisian dwellings occur elsewhere in Europe. There are no clues to the exact date of the pits. Finds include workshops for making quartzite objects, a number of tools, and a coarsely wrought disc of tertiary black amber or *gagate* which was used for making ornamental or cult objects. A caved-in furnace was also found; this was used for heating liquids in leather [*sic!*] receptacles, since the people of the period had no knowledge of pottery. On the other side of the lake a pit dwelling was brought to light, together with an outdoor fireplace, several stone slabs on which flint implements were fashioned, and several lesser pits which were apparently used for supplies. It is interesting to note that the raw material for the quartzite objects and the flint implements was apparently not of Bohemian origin.

Somewhat later are the finds at the site of the old Slav fortress at Starý Lohet on the left bank of the Ohra, some 2½ miles from Karlovy Vary. Here were found a number of stone relics of paired huts of the middle period in the shape of a series of visible post holes with traces of fireplaces or hearths between each pair of huts. The site as a whole yielded few implements, but some 3,000 implements were found in the post holes on the outskirts. These huts are known as Tašovice dwellings after the nearby parish, and are analogous in type to those found at Hamburg, though the most striking parallel

#### Note

The following publications of Professor Read are noted as bearing upon the subject of this paper:

Inaugural Lecture in the University of London, June, 1950 (to be published).

Paper read at the International Symposium held at Bristol, April, 1950, and published in *Principles and Methods of Colonial Administration*, Butterworth's Scientific Publications, 1950.

George Cadbury Memorial Lecture, Birmingham University, March, 1951.

Paper read at the Columbia University Conference on Educational Problems of Special Cultural Groups, August, 1949 (to be published).

Review in *Africa*, April, 1951, of *Umbundu Kinship and Character* by G. M. Childs.

is that of the mesolithic 'villages' of southern Germany. Here too were found a number of articles made from substances obviously imported from Bavaria, and these formed the subject of detailed discussion by F. Prošek.

Drs. V. Špurný and B. Soudský spoke on neolithic finds. Here we have to deal with a culture which is markedly more advanced; its bearers were farmers of about the third millennium B.C. At Lobeč near Mšeno, Mělník, at the foot of Mount Vrátno, the object of study was one of the largest settlements of this period. This culture is founded on a loess mound covering an area of some 100-120 acres. Labour shortage prevented excavation on the scale originally planned, but the results were notable. A quantity of flint implements and other artifacts of Baltic chalk-flint, local flint and chalcedony came to light, in addition to a quantity of late stroke-ornamented ware. Finds of sickle-shaped stones and nether mill-stones testify to an agricultural way of life. Dr. Soudský went on to speak of some newly discovered traces of late dwellings in the sandy terrain round Postoloprty. The credit for this discovery goes to the students of the Prehistoric Institute of Charles University, since it was they who found the post holes outlining a building of some 41 metres by 7.5 metres. It used to be said that these neolithic structures were granaries. Recent Soviet excavations of neolithic villages have shown however that these elongated buildings were residential and formed an unbroken outline round the village. It is hardly likely that a people acquainted with advanced methods of farming would live in pit settlements while constructing spacious buildings as granaries. Of this habitation there survives of course nothing more than the post holes, which contain fragmentary remains of charcoal. Supported by the posts there must have been a roof, presumably of reeds or other material. Further observations are needed before final judgment can be passed with regard to these buildings.

#### II

Dr. J. Neustupný opened the third evening session of the archaeological working party for the current year in Prague by speaking of the progress made in the explorations at Hluboké Mašůvky near Znojmo. After a number of excavations had been made dating from 1947 a large fortified settlement came to light dating from the third millennium B.C. It is a neolithic site. The whole settlement was surrounded by an elliptical moat which has not yet been completely laid bare, but last year's work uncovered about 400 metres of its total length, which is estimated at some 950 metres. It is not certain whether the whole of the moat can be traced as part of it may have been ploughed out. Its prehistoric builders clearly had considerable trouble with this ellipse and were unable to keep its course even at all points. The most interesting features are of course the entrances to the settlement, where the moat was interrupted. Pile erections obviously dominated the entrances, and the moat was further reinforced at these points by a double row of palisades between which clay had been deposited. But the clay rampart is somewhat puzzling. To one side of one of the entrances it extends for 26 metres, to the other it runs for only 10 metres. The remainder of the bank, insofar as it probably extended behind the moat, may have been simpler,



but its characteristic traces have been lost. The fact that the pit settlements so far excavated belong to two cultural ages make interpretation difficult. Spiral-meander ware belongs to an earlier period, Moravian painted ware to a later. Last year's excavations make it clear beyond doubt that the fortification was late, and that spiral-meander folk had once inhabited the place without fortifications. The fortification shows that this prehistoric settlement was of high civic and economic importance. Analogous fortified settlements are to be found in other parts of Moravia, in Germany, southward over the Balkans, in Asia Minor and Mesopotamia. The so-called 'Troy II' is of this type. Moreover the culture-bearers of the unfortified settlements were clearly of minor importance.

L. Hájek spoke of recent discoveries of bell-beaker settlements at Lhánice. Here 12 graves, two with cremation burial, were found, which date from about 2000 B.C. Though they are still of the stone age, the earliest metal weapon occurs, viz. the dagger, indicating the approach of the bronze age. The bones of young sheep were found in each of the graves, and in each case one specific part of the sheep was buried with the corpse. Burials were always made with the body facing east, and in addition to the pieces of mutton, various utensils were put into the graves, bell-beakers being typical of these. Among the finds was a kind of strainer used in conjunction with some kind of textile through which some liquid was passed. Traces of wooden beams in the graves indicate that they were originally roofed. There are various indications that the people were pastoral. They bear witness to various kinds of division of labour, but the graves show as yet no sign of social stratification. These people bore some relation to the older folk of the painted ware; they may have dominated the latter through their knowledge of the earliest bronze weapons. They were also acquainted with the first gold and silver ornaments. The recent finds, once fully studied, may well lead to a revision of present views on the dating of neolithic cultural strata.

**African Abstracts (Bulletin Analytique Africaniste).** A note on the first six issues by G. W. B. Huntingford

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In MAN, 1950, 7, there appeared a preliminary notice of this periodical, which has now completed six issues (January, 1950, to April, 1951). It is published by the International African Institute (price £1 6s. a year) and edited by Professor Daryll Forde, with Miss Ruth Jones as Assistant Editor. Its object is to provide abstracts in English or French of papers relating to the ethnography, sociology and linguistics of Africa which appear in journals all over the world. The first volume covers material published in French, Flemish, Spanish, Portuguese, Italian, German, and Russian, as well as British and American journals. The continent is divided for the grouping of ethnological and social studies into nine main regions: Africa, general; Northern (Libya, Spanish Morocco, Tripolitania, etc.); North-eastern (Ethiopia, Eritrea, Somaliland, Northern Sudan, Socotra); Western (French West Africa, Togo, British West Africa, Cameroons, Liberia, Portuguese Guinea, Spanish Guinea, Fernando Po); West Central (French Equatorial Africa, Cameroun, Angola, Belgian Congo); East Central (Kenya, Ruanda-Urundi, Southern Sudan, Tanganyika, Uganda); South-east Central (Nyasaland, Northern Rhodesia, Southern Rhodesia, Mozambique); South (Union of South Africa, South-west Africa); and Madagascar. For linguistics the division is: General and Northern; Berber languages; languages of North-east Africa;

languages of West Africa; Bantu languages; Malagasy. A list of the periodicals from which abstracts are made is given in each quarterly issue; there is an index of subjects (in French and English), tribes and languages, authors, and periodicals abstracted.

Papers described by the editor in the introductory editorial as 'marginal' or 'superficial' receive only a brief notice; others are summarized with a fair amount of detail. The amount of space allotted to the more important articles is generally adequate, though in some cases (e.g. Vol. I, no. 382, History of the Samba) it is obvious that such papers as a tribal history of 36 pages cannot be abstracted in any sort of detail, and the 11 lines devoted to no. 382 are really as much as can be expected. As examples of particularly full summaries we may mention nos. 56 (Material life of the Brame), 117 (Modern political organization of the Plateau Tonga), 245 (Étude sur les devins... de Mushie), 410 (Masai social system), 445 (Little known tribes of the Bahr el ghazal), and 537 (Fouilles dans la région du Tchad) (all from Vol. I). These range from 500 to 1,200 words. I have singled them out simply to show the amount of detail that may be given where necessary. Some of the shortest notices do not exceed 30 words.

The virtue of this collection of abstracts is that: (1) it not only gives a clear *resumé* of current literature forming a useful adjunct to such periodical bibliographies as those already published in Africa and elsewhere, but also provides a clue as to whether papers in journals difficult of access are worth following up; (2) it is particularly valuable in providing a knowledge of current foreign periodical literature; (3) it should be of value to those living in Africa out of the reach of libraries; (4) it includes material dealing with modern economic, administrative and social problems that is not always seen by the ethnographer.

It would be interesting to know the principle on which papers are chosen for abstraction. I mention this because after going through MAN I find that of the 24 papers dealing with Africa which appeared there during the period covered (January, 1948, to July, 1950), eleven are not abstracted; and in Vol. II, no. 2, there appears an abstract of MAN, 1950, 133 (on the Nuer), but not of the paper which immediately follows it (no. 134 on the Bemba.) It is of course realized that the number of periodicals and papers abstracted must depend to some extent on finance; but may one hope that in future issues such periodicals as *Archæologia*, the *Geographical Journal*, the *Journal of the East Africa Natural History Society*, and the *South African Archaeological Bulletin* will appear in the list when they contain relevant matter? It is also to be hoped that papers dealing with Africa that sometimes appear in specialist journals not connected with Africa will find their way into *African Abstracts*, e.g. journals relating to India and the Middle East.

One or two curious things are revealed by these abstracts. Why, for example, should a not very accurate anonymous paper on the Somali appear in an official Southern Rhodesian journal? And a denunciation of the 'Hamites' and denial of their existence, apparently because they are 'bourgeois', comes from a Russian periodical.

The International African Institute is to be congratulated on the production of a very useful periodical; and the abstractors have, so far as I can tell, done their work well.

## REVIEWS

### AFRICA

**L'Archéologie de l'Afrique Noire.** By D.-P. de Pedrals. Paris (Payot), 1950. Pp. 234, illus. Price 500 francs

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The serious student of African archaeology may well envy the good fortune which has enabled M. de Pedrals to publish this book; though it is doubtful whether his reputation as an archaeologist will be increased by its publication. I opened

the book eagerly, expecting to find a survey of what is known of the types and distribution of archaeological remains in Africa. I have seldom been more disappointed. In 230 pages the author has succeeded in missing an opportunity which, as I have said, others will envy him. There is so far no book which covers the archaeology of Negro Africa, and instead of giving a methodical presentation of



the facts, the author has chosen to fritter away his space with muddled descriptions that fail to give a clear picture; and with 80 pages of would-be scientific discussions of names and words that add not one jot to our knowledge of African archaeology.

The first part divides Africa into seven archaeological regions, to each of which a chapter is devoted. Chapter I deals with Nubia, i.e. Napata and Meroe, twelve of the 28 pages devoted to which are occupied with wild discussions of such things as the names 'Kham' and 'Koush,' but omitting any mention of the ruins in Darfur described by Arkell in *Sudan Notes and Records*, 1936 and 1946. In Chapter II there is a muddled account of the archaeological material in Ethiopia (Abyssinia), with a mention of the Konso wooden figures, but not of their megalithic cult. The chapter ends with seven pages of useless discussion. There is no attempt at a classification of the types of monument found in North-East Africa, and no suggestion of any connexion with Arabia, still less any mention of the parallels with South-East Asia suggested by Neuville as long ago as 1932. Chapter III deals with *L'aire du Niger Sahélien* (the central Nigerian plateau), with a five-page discussion on historical MSS. that is not adequately brought into relation with the text. This chapter also ends with a wordy discussion of serpent myths, giants, names, and legends. Chapter IV, on the Chad area, contains descriptions of excavations and lists of pottery types, with such expressions as '*de petits godets assez grossiers*,' mixed with a jumble of historical and ethnographical extravagances. Chapter V, *L'aire du Niger inférieur*, deals with the finds from Ile Ife, based on Frobenius, and among other things identifies Kisra with Chosroes (Khusru) II. The author evidently believes the statement of Mohammed Bello that twelve Manding kings were Persians, for he seems to accept as 'proof' the sacrifice of a black bull, which he says is directly derived from the cult of Mithra. The Zambezi area is the subject of Chapter VI: here we have a long and confused account of Great Zimbabwe, with a bare mention of Namatali and five other sites, which are dismissed as being '*de moindre importance*.' This is followed by an unenlightened discussion of origins. There is no mention of anything else in East or South-East Africa. The '*Documents archéologiques d'autres lieux*' of Chapter VII are the stone circles and other megaliths of Senegambia, based on Jouenne, whose more recent work is ignored, both in the text and in the bibliography; no mention is made of Parker's paper in *J. Roy. Anthropol. Inst.*, Vol. LIII (1923). A clue to the author's mind is given on p. 134, from which it would appear that phallic stones occurring in Senegal and Abyssinia must be 'closely associated' just because they are phallic. The unmethodical and unilluminating account is followed by a three-page description of a '*terra cota*' figurine from Léopoldville which has no apparent connexion with anything that has preceded. These seven chapters occupy the first 140 pages of the book.

The rest of the book, pp. 141-223, contains ten more chapters divided into four parts entitled '*Les Ethnonymes*,' '*Les Toponymes*,' '*Les Théonymes*,' and '*Archéophilologie*.' Most of this is what might be called mere comparative speculation, completely uncritical, and with no bearing on the archaeology of Africa. Dahomey, for example, is Dan-homey, and derived from the Biblical Dan (pp. 170-76).

There are 15 text figures drawn by the author (the title page says 68 drawings, but I have not counted them), of a poor quality, some, like Fig. 5, no. 5, and Fig. 15, so ill drawn that they fail to achieve any purpose. There is no map, a very serious omission in a book which, from its title, purports to cover the whole of 'Negro Africa'; nor is there an index, another equally serious fault. Moreover, only a restricted part of the continent is actually covered by the book: Abyssinia, Nubia, part of Southern Rhodesia, and West Africa between Lake Chad and Senegambia. The rest of 'Negro Africa' is simply ignored. An introduction to the archaeology of Africa is much needed, but this book does not supply that need. What is required is a survey of the existing remains, with a classification of types of monument, adequate bibliographies, and no more theorizing than is necessary to place the different groups in relation to each other. Philological discussions such as those offered by M. de Pedrals are out of place. Scientific studies of such things have their place, but that place is not here. This book cannot be recommended even as being 'better than nothing': the serious student of African archaeology, especially the beginner, will be safer without it.

Among misprints I noted Erastosthènes for Eratosthènes, p. 25; Kouso for Konso, p. 45; Kreuker for Krencker, p. 47; Aposmation Geographica for Apospasmata, p. 165; and Conti for Conti-Rossini, p. 203. The bibliography is poor, and among the names omitted are those of Bent, Dows Dunham, Leo Fouché, R. N. Hall, Hambly, Ad. E. Jensen, Leakey, MacIver, Neuville, Henry Parker, Puccioni, van Riet Lowe; some are mentioned casually without references in the text, but the titles of their works are not given. There is no mention of Schofield's useful 'Survey of Recent Prehistory of Southern Rhodesia' (*S. Afr. J. Sci.*, Vol. XVIII, pp. 81-111), which deals with Zimbabwe and associated ruins; nor of Gillman's 'Annotated List of Ancient and Modern Stone Structures in Eastern Africa' (*Tanganyika Not. and Rec.*, 1944). The author includes a fair number of French works, but even there he is not always up to date; and some of the books cited, like Beech's *Suk*, are brought in for philological reasons only. He quotes Morié's fantastic *Histoire de l'Éthiopie* but makes no mention of Budge's *History of Ethiopia*. While he has some idea of the archaeological regions of French West Africa (though he presents them badly), much of the rest of Africa appears to be a sealed book to him. There is still room for an introduction to African archaeology. This book is not even in the running.

G. W. B. HUNTINGFORD

**The Stone Age Cultures of Northern Rhodesia.** By J. Desmond Clark, with a chapter on the geology by F. Dixey, and Appendixes by H. B. S. Cooke, L. H. Wells and Geoffrey Bond. Cape Town (*S. Afr. Archaeol. Soc.*), 1950. Pp. 157, 57 plates and figs., 1 folding map. Price 21s.

'Up to about ten years ago Northern Rhodesia featured largely as a blank on the prehistoric map of Africa,' says Sir Gilbert Rennie in his Foreword to this book. In remarkably few years Dr. Desmond Clark has worked out the Stone Age sequence in this important region. Not only has he brought to light a wealth of new material, but he has studied it so thoroughly and with such careful regard for the parallel developments in other parts of Africa (as well as for the geological background) that he has been able to produce a book which can only be regarded as an indispensable work of reference for all students of the African Stone Age. It is complementary to the recently published work by Neville Jones on the *Prehistory of Southern Rhodesia*.

In Part I Dr. Dixey gives a detailed account of those aspects of the geology of the Upper Zambesi valley which are relevant to the study of prehistory. This will be particularly valuable for students visiting or working in the country. Even the general reader will find points of interest in this chapter, which includes notes on terminology which are necessary to a full understanding of later parts of the book. It is impressive to read that six miles of the great Zambesi gorge system was carved out during the third pluvial period. Admittedly this erosion was facilitated by shatter-belts in the basalt, but even so this indicates a period several times as long as the post-Pleistocene.

Dr. Dixey gives a useful summary of the local Pleistocene changes of climate. This all-important background of the cultural succession is dealt with in more detail by Dr. Clark in a later chapter on correlation. The climatic sequence in Northern Rhodesia proves to be remarkably similar to that revealed by studies in the Vaal River, in the Congo basin and in East Africa. However, it would be unwise to assume exact synchronism. Radiocarbon dating in North America has shown that in the time of onset of a particular climatic phase in the post-glacial sequence there was a timelag between Virginia and Maine of some 3,000 years. If the swing of climatic belts was comparably slow in Africa it is quite possible that they were sometimes outstripped by culture spread. This would 'make hay' of detailed archaeological correlations based on the assumption of exact synchronism, especially in the later stages of the Pleistocene when cultural change was rapid. Dr. Clark is well aware of the difficulties and his correlations are accompanied by due reservations.

The archaeologist is frequently in debt to the geologist, but in recent years particularly in Africa the balance has been redressed in the field of Pleistocene geology. For example the dating evidence of artifacts in the Zambesi gravels proved very helpful in working



out the history of the gorges and the Victoria Falls. Dr. Bond discusses another instance, the dating of the Kalahari Sand in Northern Rhodesia: until Acheulian implements were discovered below them, the antiquity of the earliest recorded spread of the sands in that region was quite unknown.

In Part II Dr. Clark describes the stratigraphy and cultural succession on a regional basis, illustrating his account with sections of the key sites, and with diagrams of the stages in the Pleistocene history of the Upper Zambesi. There is an important chapter on the archaeological nomenclature now used, and on the raw materials of Rhodesian stone industries (the cultures show increasing selectiveness).

The major portion of this book is devoted to a description and a discussion of the Stone Age cultures represented in the country. The treatment is admirably systematic. Suppose, for instance, that one is interested in the Acheulian culture. Under 'Early Rhodesian Acheulian' one finds notes on Stratigraphy, Geological Horizon, Climate, and Implements (with subheadings: Hand-axes, Cleavers, Flake Tools, Technique). Under Middle and Late Acheulian comparable details are listed, and regional variants are described; and so on throughout the long sequence of cultures. A selection of artifacts of all the industries is illustrated, through the skilful penmanship of Mrs. Clark. The source of every drawn specimen is indicated.

Two early cultures are proving particularly interesting from an anthropological point of view, the Hope Fountain and the Sangoan. The former can be shown to have been contemporaneous with the Chelles-Acheulian; it is represented by a crude industry comprising a great variety of small flake tools, utilised cores and rough choppers. The author classifies the flake tools according to shape and character of the working edge, avoiding any reference to possible function. One can but admire the restraint which he shows in not calling this culture Clactonian. He is wise not to do so yet; but the similarity is so close that one cannot help feeling now that the Clactonian-Tayacian cultures of Western Europe have just as much claim to be attributed to a tradition of African origin as they have to be regarded as ends of an Asiatic (Choukoutienian) culture stream.

The Sangoan culture appears to have originated in the Congo and to have spread into Rhodesia during the dry period which followed the second pluvial, when forests gave place to savannah. Sangoan industries (distinguished by pick-like tools) are not only more widespread than those of the Chelles-Acheulian culture, but more abundant, indicating a big increase in the population of Rhodesia at this time. The author regards the culture named Bembesi by Neville Jones in Southern Rhodesia as a local variant of the Sangoan.

When such a feast of interesting data is provided it must seem churlish to ask for more; but if a second edition is published one would hope to see a fuller account and some figures of the Proto-Stillbay industry which, as Dr. Clark recently showed in the Institute's *Journal*, was associated with *Homo rhodesiensis*.

Late Stone Age industries hitherto found only on the surface of the Tanganyika plateau region have recently been found stratified in caves in the Mpika district of north-east Rhodesia. Dr. Clark proposes the term 'Nachikufu culture' to cover them. These industries differ from the Northern Rhodesian Wilton in the abundant occurrence of *tranchet*-type microliths, bored stones and scrapers of Smithfield types. The culture is associated with a basically naturalistic art tradition and with grooved rocks marking the sites where stone celts were ground and polished. Like the Smithfield culture of South Africa it probably stemmed from the Congo.

Dr. Cooke provides a useful commentary on the Quaternary mammalia found in the Victoria Falls gravels and in the Broken Hill and other caves. Dr. Wells discusses the remains of fossil man from this country, which include several interesting Bushmanoid and pre-Negro skulls in addition to the famous specimens of *Homo rhodesiensis*. The reference of these last to an early stage of the 'Middle Stone Age' indicates that it was probably contemporary with Upper Palaeolithic Man in Europe. Dr. Wells appears to favour the view that this species was a highly specialized offshoot of the ancestral australoid stock from which modern African types were derived.

The South African Archaeological Society is certainly to be congratulated on the publication of this lavishly illustrated and well printed volume at such a low price. KENNETH P. OAKLEY

**The Calendars of Ancient Egypt.** By Richard A. Parker. *Oriental Institute, University of Chicago, Studies in Ancient Oriental Civilization*, No. 26. Chicago, Ill. (Univ. of Chicago Press), 1950. Pp. 83, 6 plates, 21 text figs. Price \$6

The studies of the Egyptian calendar have by now reached an imposing number, but not unnaturally they are full of mistakes and misunderstandings. The present work points this out in many instances and puts together the information known at present, including some that is new. Every period has been laid under contribution.

As opposed to the complicated and abstract calculations so often propounded, the author starts from the simple probability that, as with all primitive peoples, the self-evident lunar month is the basis of the Egyptian calendar. He also takes the self-evident fact that the farming operations, so evident in the construction of the calendar, were dependent upon the rise of the Nile. This however could vary greatly in date. Already by the beginning of the First Dynasty it had been noticed that Sirius (Egyptian Sothis), the brightest star in the firmament, was accustomed to rise about the time that the rise of the Nile was due. Hence, as time passed this occurrence was taken as 'The Opener of the Year.' His rising happened to be constant, which enabled calendar-making to be started. That is all that Sirius had to do with the Calendar. Moreover, modern speculations about the summer solstice, etc., or indeed any solar connexion with the calendar, are baseless.

There was little or no difficulty until things became organized. The problem then was how to fit months into a year. However the Egyptians may have arrived at the number, and it does not yet seem clear, they fixed on 365 days. In so doing they unwittingly started, but never used, what we now know as the Sothic Cycle. For us however it is of prime importance, for it enables us to calculate some definitely fixed dates.

The Egyptian day began at dawn, and was thus independent of the Babylonian reckoning which began it with the evening. While the Babylonians, like most people, reckoned from the New Moon, the Egyptians reckoned their month from the morning that the old crescent was no longer visible. The only other people known to do so are the Masai and Wachagga. In Loango the lunar year is corrected by the rising of Sirius, apparently a unique method in the modern world. This no doubt, like the two African methods just mentioned, must be a survival from Egypt—the only country where Sirius was of real importance.

G. A. WAINWRIGHT

**Ethnographic Survey of Africa: East Central Africa, Part I: Peoples of the Lake Nyasa Region.** By Mary Tew.

O.U.P. (for Int. Afr. Inst.), 1950. Pp. x, 131, folding map and 2 text maps. Price 10s. 6d.

**Part I: The Southern Lunda and Related Peoples (Northern Rhodesia, Angola, Belgian Congo).** By Merran McCulloch. London (Int. Afr. Inst.), 1951. Pp. vi, 110, folding map and 2 text figs. Price 8s. 6d.

The aim of this survey, as stated in the Preface by the editor of the series, Professor Daryll Forde, is to 'present a concise, critical, and accurate account of our present knowledge of the tribal groupings, distribution, physical environment, social conditions, political and economic structure, religious beliefs and cult practices, technology and art of the African peoples.' That is to say, it is a compilation from existing works, and one of its functions is to point out the gaps in our knowledge, as well as to summarize what is known. In general, these two books have been competently prepared, and, subject to the limitations imposed by their sources, they offer a useful synthesis of the ethnography of the region covered. It is to be hoped that before long we shall have the complete series.

In the first book under review, Miss Tew surveys the Yao, Makua-Lomwe and Makonde in the Mozambique region; the Maravi peoples (Nyanja, Cewa and Nsenga); the Tumbuka and lake-shore Tonga; and the small scattered Ngoni groups. In the second, Miss McCulloch deals with the southern Lunda and Ndembu; the Chokwe and Minungu; and the Luena, Luchazi, Luimbe and Mbunda; the area covered being eastern Angola, south-west Belgian Congo, and part of Northern Rhodesia east of Barotseland.

Though drawn up on a similar plan, the arrangement is not



exactly similar in both these books, and facts are more easily found, I think, in Miss Tew's book than in Miss McCulloch's. The subjects included are arranged thus: after a short account of tribal nomenclature, grouping, history, demography, linguistic position, and physical environment, the headings in Miss Tew's account are: (i) Main features of economy (traditional economic life; division of labour; cultivation; cattle; domestic architecture; land usage; craftsmanship; warfare; labour migration). (ii) Social organization and political structure (kinship and local grouping; betrothal and marriage; inheritance; slavery; chiefs, etc.; justice; modern administration; land tenure). (iii) Main cultural features (religion; sorcery and/or witchcraft; birth; initiation; ritual taboos; menstrual and other taboos; burial rites; 'utani'; other features, e.g. facial cuts, lip plugs, wood-carving). The grouping is similar in Miss McCulloch's book, though the order is not quite the same, 'religion' and 'the life cycle' being dealt with separately. It would be easy to quarrel with this arrangement: some of the features might be grouped a little more systematically, and in a second edition both arrangement and terminology should be carefully scrutinized. The term *utani*, for instance, does not appear to be defined, and the absence of an index makes it difficult to find quickly the references to it.

The value of a survey such as this depends, apart from the method of presentation and the accuracy with which it is prepared, upon whether it tells the enquirer what he needs to know (within reason, of course, for the space is limited); and whether what is in it can be found easily. As to the first point, these two books have, I think, achieved what they set out to do; if the treatment of some subjects is unequal, the meagreness of the sources may be blamed. But the treatment of material culture is poor, and not always up to date (e.g. the Makonde wood-carving is dismissed in two lines (Tew, p. 29), and the paper on it by H. D. Collings in *MAN*, 1929, 17, does not appear in the bibliography; this is but one example). The treatment of such culture features as lip plugs is inadequate, and should have had at least a distribution map. The preparation of this survey must have given an unparalleled opportunity of examining the more obvious (but not for that reason unimportant) aspects of material culture, and a greater amount of relevant information on this subject should have been included.

As to the second point, books like these need both an index and a detailed table of contents; if the latter is really detailed an index becomes less essential, though it is still desirable. Neither book has an index, and the tables of contents (especially in Miss Tew's) are not sufficiently detailed. The map in Miss McCulloch's book is not clear enough; both the blue and the black should have been heavier.

Visible evidence of the critical aspect of the treatment (mentioned by Professor Forde in his Preface) occurs in Miss Tew's useful bibliography, where the subject and/or value of each entry is briefly noted. No such critical aid is given in Miss McCulloch's bibliography.

It is unfortunate that Miss McCulloch's book has had to be printed by a 'near-print' reproduction process; this does not, however, reduce the value of the book. Both the International African Institute and the authors are to be congratulated on the production of two useful books which will help students of the regions concerned to see more clearly the differences and relationships between the peoples of a complicated region, and to estimate the range of future fieldwork.

G. W. B. HUNTINGFORD

**Ethnographic Survey of Africa: Bemba and Related Peoples of Northern Rhodesia.** By Wilfred Whiteley, with a contribution on the Ambo by B. Stefaniszyn, S. J. **Peoples of the Lower Luapula Valley.** By J. Slaski. In one volume. London (Int. Afr. Inst.), 1951. Pp. 100, maps. Price 9s. 6d.

This volume is Part II of the East Central Africa section of the Ethnographic Survey of Africa, and follows the same general plan as other volumes in the series which have already appeared. It provides a survey of the information available for a large number of peoples in Northern Rhodesia, and the adjacent portion of the Belgian Congo, who speak dialects of what may be called the 'Bemba' language. The first section, written by Mr. Whiteley, deals with six different groups: (1) Bemba, Bisa, Aushi, Unga, Shila, Tabwa, Bwile, Twa; (2) Lala; (3) Ambo; (4) Lamba; (5) Kaonde; (6) Senga. The second section, by Mr. Slaski, covers the people

living in the lower Luapula Valley, who are of diverse tribal origins.

It is impossible to criticize the classification into six groups followed by Mr. Whiteley, since the reasons for the classification are not given.

Published material is inadequate to provide any real description for most of the peoples covered; for only the Bemba and the Lamba have been described at length. Less adequate descriptions exist for the Unga and the Kaonde. The sections on the Lala and on the Luapula peoples are based largely on unpublished material provided by Mr. D. Peters of the Northern Rhodesian Agricultural Department and by Mr. I. Cunison of the Rhodes-Livingstone Institute. Father Stefaniszyn himself summarized his unpublished work on the Ambo, and this is a valuable addition to our knowledge of the area. The sections on agriculture for all the groups are good, based as they are on the invaluable Ecological Survey of Northern Rhodesia. Little information is available for the Bisa, Aushi, Shila, Tabwa, Bwile, Twa and Senga, and what there is has been largely eliminated in the attempt to compress the volume within its present length. The section on the Senga, for example, consists of two pages, one of which deals with agriculture and draws upon the Ecological Survey, although the bibliography contains a reference to an unpublished manuscript by E. H. L. P., entitled 'The Customs and Beliefs of the Senga.'

The authors cannot be criticized for the deficiencies of the materials with which they have had to work, and it is not clear whether they can be held responsible for the general treatment of such information as they had at their disposal. In his foreword, the Director of the International African Institute declares that the material is to be presented on as consistent a plan as possible. Now it is obvious that complete consistency is impossible because of the varying nature of the material available, but there are a number of inconsistencies in this volume alone—even more striking when other volumes of the series which have already appeared are compared with one another—which suggest that those in charge of the series might usefully spend some time in considering just what should be covered by the survey. Mr. Slaski and Father Stefaniszyn, for instance, list the different missions at work in the areas which they cover; Mr. Whiteley does not, although he refers to changes in Bemba custom brought about by mission influence. Mr. Slaski reports the presence in the Luapula Valley of the *banucapi* movement, and refers to Dr. Richards' article describing the movement among the Bemba; Mr. Whiteley does not mention the *banucapi* in the section on the Bemba. Father Stefaniszyn, in his section on the Ambo includes a number of paragraphs on funeral ceremonies; Mr. Whiteley describes for the Bemba only the ritual performed for a dead chief, and says nothing at all about funeral ceremonies among the Lamba, although Dr. Doke in his book on the Lamba devotes one chapter to funeral ceremonies, including those performed for chiefs; for the Kaonde, Mr. Whiteley says only, 'Funeral ceremonies are also occasions for ritual but there is no information on the significance of a chief's burial.' Father Stefaniszyn describes rules of land tenure among the Ambo; Mr. Whiteley and Mr. Slaski ignore these save in connexion with a discussion of hunting and fishing. Mr. Slaski has a small section on diet, a subject not discussed by Mr. Whiteley, although Dr. Richards has provided the information for the Bemba, and Mr. Peters' unpublished material was available for the Lala.

Similar variations in treatment occur throughout the volume, and raise the question as to whether the Ethnographic Survey is providing a concise but comprehensive summary of the material available on the different African groups.

A number of minor criticisms may be made. Mr. Whiteley uses 1933 population figures for the Bemba group, although later figures must be available. It is sometimes difficult to discover whether the customs described are those of the present day or of many years ago. In the section on the Lamba, Mr. Whiteley has a paragraph entitled 'Administration,' throughout which he uses the present tense, although he speaks of the powers of the chiefs as including the right to inflict punishments such as fining, slavery, mutilation and death; no chief in Northern Rhodesia has exercised such powers for a good many years. In the same section, in a discussion of the duties of village headmen, it is stated that 'The



headman also regulates the payment of tribute to the district chief,' although Döke, whose book on the Lamba was published in 1931, speaks of this tribute as a thing of the past.

Each section of the book has its own bibliography. The one for the Luapula area has been carefully annotated, and is thus doubly useful. It is to be hoped that those in charge of the Survey will see to it that in future volumes all bibliographies will be annotated. The one attached to the first section of the present volume is not. Incidentally, in a footnote on page 3, there is a reference to Meinhard, 1947. This item does not appear in the bibliography. A map which shows the distribution of the tribes covered in the volume would be simpler to read if it had been possible to use colours instead of black and white symbols which are not always sufficiently differentiated from each other.

Despite its limitations the volume should prove useful, especially for those making their first acquaintance with the ethnography of Central Africa. E. COLSON

**Ethnographic Survey of Africa: West Africa, Part I: Akan and Ga-Adangme Peoples of the Gold Coast.** By Madeline Manoukian. O.U.P. for Int. Afr. Inst., 1950. Pp. vii, 112. Price 8s. 6d. **Part III: The Ibo and Ibibio-Speaking Peoples of South-Eastern Nigeria.** By Daryll Forde and G. I. Jones. O.U.P. for Int. Afr. Inst., 1950. Pp. 94. Price 7s. 6d.

These volumes are two of the four sections that have so far appeared of the Ethnographic Survey of Africa, which is being undertaken by the International African Institute. The Director of the Institute writes: 'The aim of the Ethnographic Survey is to present a concise, critical and accurate account of our present knowledge of the tribal groupings, distribution, physical environment, social conditions, political and economic structure, religious beliefs and cult practices, technology and art of the African peoples.' The two volumes considered here fulfil this aim admirably and at the same time point up our lack of knowledge of certain aspects of the cultures considered. Information on the Adangme peoples, estimated by Field as numbering some 100,000, is amazingly scarce. One important point that these studies demonstrate is the great variation in social organization from region to region of a people having, or considered as having, a common culture. This diversity in large-scale societies is often overlooked by those who deal, or have had to deal, with small-scale ones, where minor divergencies become 'ironed out' in the process of abstraction of the social structure.

Each study is intended to be self-contained and the material to be arranged 'on as consistent a plan as possible.' In view of the fact that these studies do not have any index it would help those wishing to compare certain traits from two areas if the sub-categories of the principal divisions of the subject matter were included in the table of contents. The maps are extremely useful, though the convention of various combinations of dots, as used in *The Ibo and Ibibio-Speaking Peoples*, would seem to be a somewhat unfortunate cartographical device.

It should be pointed out that the bibliographies are lacking a number of titles, though no claim to completeness is made. It would seem that Joseph H. Greenberg's *Studies in African Linguistic Classification* appeared too recently for inclusion and consideration; this work offers a classification which should help to bring clarity to the subject of African languages.

Space does not allow of a consideration of the implications, the pros and cons, of this type of survey as opposed to that being conducted by the Human Relations Area Files at Yale University. They stem from different historical trends in anthropological thinking and both raise a number of important theoretical, as well as practical, problems for anthropology. Anthropologists wishing to consider such problems will immediately call to mind the Bureau of American Ethnology's *Handbook of South American Indians*.

To those interested in African problems these volumes are invaluable, but they should also be considered by all those interested in general ethnology, for they provide a convenient *port d'entrée* into several important West African cultures. Their moderate cost is a further point in their favour. PHILIP J. C. DARK

**Peoples of Sierra Leone Protectorate.** By M. McCulloch. London (Int. Afr. Inst.), 1950. Pp. 102, map. Price 8s. 6d.

**215** This book is Part II of the Western Africa Series of the Ethnographic Survey of Africa, edited by Daryll Forde, which aims to 'present a concise, critical and accurate account of our present knowledge of the tribal groupings . . . of the African peoples.'

Miss McCulloch considers the Sierra Leone peoples in four groups: Mende and Lokko; Temne, Limba, Susu and Yalunka; Sherbro, Bullom, and Krim; Kono, Vai and Koranko. Each of these groups she describes under the following headings: grouping and demography; history and traditions of origin; main features of economy; social organization; associations; and other social features. There is a bibliography of 87 references, and a well designed map showing the extent of each tribe and chiefdom. In the review copy at least, the map inconveniently faces the back of the book when unfolded.

The boundaries of Miss McCulloch's ethnographic area are of necessity more arbitrary than those of the areas covered in other studies of this series, and accounts of the Kissi and Gola have had to be excluded since the bulk of their population lies in French Guinea and Liberia. Within the area the Mende are taken as typical, owing to their extent, and, we may guess, the disproportionate amount of material written about them.

This survey maintains the standard of the whole series, and will prove useful to all concerned with social groupings in the Protectorate. DERRICK J. STENNING

**Le Héros Civilisateur: Contribution à L'Etude Ethnologique de la Religion et de la Sociologie Africaines.** By Harry Tegnæus. Stud. Ethnog. Upsaliensis, Vol. II. Upsala, 1950. Pp. 224. Price £3

**216** This is the second in this well produced Swedish series of ethnological studies. In it, Dr. Tegnæus traces through much of Negro Africa the form taken by 'culture heroes,' who are believed to have introduced men to some of the material necessities and social institutions which they now have. It would be a sort of service simply to list the forms taken by these figures, after a survey of the literature, some of which is not easily accessible. Dr. Tegnæus attempts more than this. As the title suggests, there is an effort to make the general similarities found in various African myths fit into a pattern and lead to conclusions about the history of African peoples. Like the first book in this series, Lagercrantz's *Contribution to the Ethnography of Africa*, this book conducts the reader on a rather wearying tour of sources in ethnographic literature, with little indication that the author is in any position to judge of their reliability or credibility. The similarity of certain stories told in Africa is shown, but the reader is never left long enough in one place to feel that he has really understood the nature of the similarities by understanding their specific social contexts. Like the book of Lagercrantz, too, this book tends to treat the various cultures of prehistoric African stocks, which are no more than hypotheses which might explain the present distribution of various rather arbitrarily chosen 'culture traits,' as though they were historical realities. Thus we have 'palaeonigritique,' 'néosoudanais' and so on.

There is much interesting material in this book. Perhaps it is due to Malinowski's influence—a tribute to it, one might say—that ambitious ethnology of this kind no longer seems to make a contribution at all commensurate with the labour involved, to our understanding of African societies. Although the author has referred to the best field monographs where he could do so, adequate accounts for the whole area studied simply do not exist. Moreover, the study is surely incomplete unless some indication is given of the world distribution of some of the symbolic figures here considered? Without some such indication, how can it be supposed that specifically African 'civilizations' can be posited from them?

Had the author examined his material with the same interest and care, but with the intention of drawing conclusions of a sociological nature from it, his ethnology need not have suffered, and his conclusions from a wide reading might have seemed to be something more than still further unverifiable hypotheses. As it is, what is here presented in conclusion is rather one way of examining the material.



We are left with a knowledge of the partial distribution (as far as the literature permits) of culture heroes in the form of insects, animals, human beings and spirits, and the book performs a service in ranging through the literature to find them. Yet, as to the conclusions:

'It seems a shame, the Walrus said, to play them such a trick,  
After we've made them come so far, and made them trot  
so quick.'

R. G. LIENHARDT

**Christianity and Native Rites.** By William Vincent Lucas (Bishop of Masasi, 1926-44) and E. O. James, with an appreciation of William Vincent Lucas by W. G. de Lara Wilson. London. (Central Africa House Press), 1950. Pp. 67. Price 3s. 6d.

Bishop Lucas's essay, here reprinted from *Essays Catholic and Missionary*, is an attempt to give guidance to those missionaries who want their converts to be deprived as little as possible of the satisfactions of their native cultures, while ensuring their Christian orthodoxy. There are suggestions about the extent to which native rites, in this case African rites, may be retained and modified for converts within the framework of Christian dogma. Criticism such as one occasionally hears about 'missionaries' usually fails to acknowledge the sort of missionary anxiety which prompts this essay—the anxiety of those who have a certain respect and sympathy for some of those customs which they involuntarily find themselves destroying, and even retain a sort of sympathy for those which it is their purpose and duty to stop. Professor James, writing as an anthropologist, endorses the Bishop's suggestions.

The value of the Bishop's policy can be decided only by missionaries of like mind to his who have attempted to put it into practice. It is suggested here that the purging of what Professor James calls 'crudities' from certain rites has enabled missionaries to adapt them successfully to Christian use. Still, some—and not only those who are supposed to have a romantic desire to preserve the integrity of native custom at all costs—will find themselves agreeing with the Bishop's statement that

'the education of the Christian conscience is worth far more than rigid rulings, for, unless the African conscience assents, rigid rulings will be of no avail except within earshot of the European mission station.'

Such suggestions as those about times of night at which dances ought to stop, or the selection of initiation dances from an 'approved list,' or the missionaries' attitude to a self-professed witch—a period of detention, where the Government takes cognizance of such conditions, will be no act of injustice—make the reader wonder about the reactions of some African Christians. Will they approve this foreign solicitude for their own custom, purged of what Professor James refers to as its 'gross symbolism'? Can one suppose that, after their free and independent acceptance of Christianity on the terms within which it is offered doctrinally, they will for long be able to take seriously an 'approved' and bowdlerized selection of details of their own culture, which they not only *may* keep, but in some cases, in the view of their teachers, *ought* to keep? The Bishop remarks that 'a Zulu Christian in no way ceases to be a Zulu.' Does this mean to an African what it means to the Bishop, and will a Zulu, having accepted that for his own good he ought to be a Christian, feel that the same interest in his spiritual welfare prompts the missionary who tells him that, of course, he ought to be a Zulu Christian?

R. G. LIENHARDT

**Gusii Bridewealth Law and Custom.** By Philip Mayer. Rhodes-Livingstone Papers, No. 18. O.U.P., 1950. Pp. vi, 67. Price 6s. 6d.

This is an interesting and useful study of the law and custom relating to bridewealth in a group of pastoral and agricultural Bantu tribes on the fringe of the Nilo-Hamitic area in Kenya Colony. It was written primarily as an aid to administrative officers in the decision of bridewealth cases, and should well fulfil this purpose.

It begins with a short summary of Gusii political organization,

followed by a descriptive account of the customs relating to the payment of bridewealth grouped under five headings: (i) customary procedure of the bridewealth transfer (i.e. the mode of choosing and transferring the stock); (ii) the marriage outlay (the actual composition of the bridewealth); (iii) assembly and distribution of bridewealth (the rules for the allotment of the stock); (iv) the maintenance of equilibrium (replacement and recompense); (v) legal implication of the bridewealth transfer. A number of points of interest emerge from this study; among them are: the use of a go-between in the initial stages of the negotiation, and the fact that even the 'modern' young man does not himself conduct the negotiations for the selection of the bride-cattle; the maintenance of equilibrium by the replacement of bride-cattle lost through death or other means, and the contrast between the methods adopted by government-appointed chiefs and tribal elders in dealing with replacement cases; and the possibility of divorcing a wife after she has borne a child.

The Gusii live on the edge of both the Nilo-Hamitic pastoral area and of the Bantu of North Kavirondo (North Nyanza), and this book provides some material for comparative study. Where the Bantu are concerned, we find, for instance, that while the Isukha of North Kavirondo make use of a go-between, they have no rules compelling the replacement of the stock. With regard to the Nilo-Hamitic pastoralists, a number of contrasting practices emphasize the different attitudes to cattle: the slaughter of cattle during merry-makings, and the payment of debts from stock intended as bride-cattle, are foreign to the true East African pastoralist, who on the other hand tends to require fewer cattle for the payment of bridewealth. Thus, whereas the Gusii pay from one to twenty head of female stock, and the Isukha from one to eight, the Nandi pay only one to four and the Kipsikis three. Maintenance of the number of bride-cattle is not considered essential by pastoralists like the Nandi, whose attitude to cattle is less material and more mystical; and divorce is possible among them only if the wife has not borne a child.

A book dealing with a tribal group needs a map, especially when the group is divided into contrasting sub-groups like the Gusii; and this is one of the things we hope Dr. Mayer will give us in the next instalment of his Gusii material. It is not quite clear which are the practices of Chache and Masaba (p. 2), and on pp. 9 and 11 they seem to be reversed. A more precise definition of the functions of the *enyangi* ceremonial is required, for much seems to be related to it, and the reasons for its importance are not adequately brought out. When the author writes of 'irregular marriages' (p. 12), one wonders whether 'marriage' is the right word here; and what is the legal status of the issue of such marriages?

G. W. B. HUNTINGFORD

**The Teda of Tibesti, Borku and Kavar in the Eastern Sahara.**

By Walter Cline. *Gen. Ser. in Anthropol.*, No. 12. *Menasha, Wis. (Banta)*, 1950. Pp. 52. Price 8s

This useful monograph on a little-known people is an outcome of the author's wartime intelligence work in ethnology as a U.S.A. government official. It is solely a compilation from literary sources. First is given a summary account of the physical geography of the Teda country, which is illustrated by a simple map to locate sites between Lake Chad and Kufra, Murzuk and Ennedi, as cited in the text. Then follow useful accounts of the identity and numbers of the Teda, their economy and material culture, their social organization and religion.

The Teda are a linguistic community of some 200,000, of whom those in Tibesti number some 12,000 in 40 patrilineal clans. Most Teda are nomadic, although in detail their economy varies regionally. Even in the best years Tibesti does not produce enough food; therefore, in supplement, trading or raiding is imposed. There has been a marked tendency during the last 50 years to decline of agriculture in favour of herding, while hunting is practised only by the degraded caste of blacksmiths.

The dwellings of most Teda are either permanent circular stone-walled huts with a conical thatched roof, or, for temporary occupation, elongated wooden-frame huts covered with palm-leaf matting or antelope skin.



Socially, the Teda are organized in six classes, namely nobles, commoners, vassals, blacksmiths, serfs and liberated domestic slaves. The serfs are mostly war captives, or their descendants, taken during Teda raids in Fezzan, Borku and Wadai, while usually the former domestic slaves, liberated since French occupation, are Negroes. A Teda man has as many as three wives, usually each in a different place, pasture or garden, where she can work for him.

Included in this work is an excellent bibliography of some 90 items in English, French, German and Italian, which, however, although comprehensive is not claimed to be complete.

WALTER FOGG

**The Gold Coast Census of Population, 1948: Report and Tables.** London (Crown Agents for the Colonies, for the Govt. of the Gold Coast), 1950. Pp. 422. Price 10s. 6d.

**220** The 1948 Census of the Population of the Gold Coast is the most thorough ever carried out in the country. In an introduction the Census Commissioner explains the method used in the investigation. Tabulated results set out population densities, sex and age distributions and migrations, and show ranges of education and occupations; there are sections on the fertility of women and on the total membership of the various churches.

A rigorous attempt was made to achieve high accuracy of result, enumerators being instructed to seek out new or unrecorded villages. Yet in Table 19 a number of villages in the Sunson and Demon Native Authorities appear to have been missed, or, perhaps, grouped under one name in a way likely to confuse. For example, in the 1931 Census the village of Nakpando in the Sunson N.A. is given a population of 450, and in the 1948 Census it is given as 31. Such a drop is not impossible for Konkombas are highly mobile. Yet the people of Nakpando have settled over many generations in the villages of Kpandzoli, Nabwa, Nayir, Gbaragbam and many others; these do not appear in the tables, though Nakpanbole, another offshoot of Nakpando, does appear. The figure of 31 can refer only to the four compounds of Nakpando proper; the other villages appear to have been missed.

On the other hand, 'Saboba' in the same N.A. is given a popula-

tion (1948) of 828. 'Saboba' must include at least 11 villages, even though Sobiba, listed separately, has the same claim to inclusion in 'Saboba' as have the others. If all the villages in the immediate neighbourhood of Saboba market are to be included as 'Saboba' at the present time, then the figure given appears to be on the low side and it is again possible that some villages have been missed.

Further, the grouping of villages under N.A.'s may not be satisfactory in Dagomba. There, villages which follow one of the smaller chiefs are seldom contiguous and even the large chiefdoms which rank second only to Yendi themselves contain chiefdoms in the gift of the Ya-Na of all Dagomba. The Sunson chiefdom is not a homogeneous area under the Sunson Na, that is, it is not to be equated with the N.A. of Sunson. The same is true of the Demon N.A. and the Demon Na. Yet Konkonzoli and Naloyli, which lie in the area in which the Demon N.A. is responsible for roads, are listed under Yendi N.A., possibly on the grounds that they pay tax directly to the Ya-Na in Yendi. If this error has happened at all frequently it may be difficult to find some villages in the tables.

Finally, the table on fertility of women. The samples were carefully randomized and the figures presented refer to the Gold Coast as a whole, to the Colony, Ashanti and the Northern Territories. Yet even within the Northern Territories there is great cultural diversity, which is not allowed for in a random sample. In Dagomba a woman may be absent from her husband's house for up to three years after the birth of a child and sexual relations are prohibited to her; Konkomba women do not leave the husband's house after bearing a child, but there is a ban on sexual relations between husband and wife until the child begins to try to walk. There is a marked difference in the duration of the prohibition of sexual relations after childbearing between these two societies. There may even be a marked difference in the duration of a period of sexual abstinence within Dagomba itself between Muslims and non-Muslims. The value of this particular table appears to be doubtful if detailed knowledge is required.

Apart from details of accuracy and method this Census is a great advance on all previous Censuses and is a valuable work of reference to those interested in West Africa.

DAVID TAIT

## ASIA

**Child Problems Among the Arabs.** By Hilma Granqvist. Copenhagen. (Soderstrom; Munksgaard), 1950. Pp. 336. Price D.Kr. 30

**221** This is the fourth volume of a series which Dr. Granqvist has written on material collected during three years' residence in a Palestinian Arab village. Her earlier works established a reputation for admirable fieldwork. This latest volume again contains a wealth of excellent data, and is an important contribution to the literature on Arab peoples.

One of the main defects of the book is the almost exclusive use of one informant, Alya. It is a serious error to believe that one person knows a whole culture, and an even greater one to rely on an informant's explanations. Credulity, while not entirely undesirable in the field, is a fault in publication. Throughout the book, Alya is all too often allowed to give her explanation, and the matter is left at that. While Alya might know more about the evil eye, unlucky names, sacrifices and so forth, in her own particular village, this is not the same thing as saying that she has sufficient knowledge of these processes in general to make her explanations valid. In using her informant to the extent that she has done, Dr. Granqvist has assumed the validity of informants' explanations. My criticism, in short, is that we hear far too much of what Alya thinks, and too little of what Dr. Granqvist makes of what Alya tells her.

When Dr. Granqvist does theorize, it is not always possible to agree with her. The somewhat novel explanation given for polygyny, for example, can hardly be sustained. 'One reason' (indeed, the only reason offered; see p. 182) 'for polygyny and divorce is the weakness of old men for young women.' Predilections of this kind are by no means absent among the elders of our own society, but we do not have polygyny as a form of marriage.

Suspect paternity, and the mode of referring to a man as the son

of his mother, seem to have caused some difficulty. We cannot agree that the reason why men are referred to as sons of their mothers is because paternity is suspect. Whether a man is referred to as his father's or mother's son will depend on the context. When agnation needs to be stressed, reference is to the father or a male ancestor. When a man finds it necessary to distinguish between himself and his paternal half-brother, it is convenient for him to describe himself as the son of his mother. In other words, in a polygynous patrilineal society, reference to maternal origin is the device used to distinguish between agnates.

It is not, again, very clear whether the author always distinguishes between myth and history. The tale of Abu Artas, for instance, is given as historical fact (p. 189). Abu Artas is said to have killed a man whose wife was pregnant, and she subsequently bore a son. This son grew up, and one day quarrelled with another man. During the quarrel, villagers scorned him saying, 'Dost thou strike this one? No, take revenge on Abu Artas.' Now these details are apparently accepted as historically true, but it is odd that these very same details are given by Arabs elsewhere, and are often favoured ingredients in many feud histories. Indeed, it is a safe rule that, whenever, in Arab histories, a man is said to have died leaving a pregnant wife who subsequently gives birth to a son, the history must, to say the least, be regarded as suspect. But the story, which I have not quoted in full, is used here as an example of the tragedy of old age. It would have been much more useful to look at it as a rationalization of contemporary relationships between groups of people.

The word 'patriarchal' is invariably used when 'patrilineal' is meant. Finally, it is to be regretted that many of the illuminating proverbs and sayings could not have been given in Arabic.

E. L. PETERS



**The Comparative Archeology of Early Mesopotamia.** By A. L. Perkins. *Studies in Ancient Oriental Civilisation* No. 25. Chicago (Univ. of Chicago Press), 1949. Pp. xix, 200, 20 text figs., 3 tables and a map. Price 88

Miss Perkins describes this book as a study which 'synthesises all the relevant published data bearing on Mesopotamian archeology from earliest times to the onset of the Early Dynastic Period.' It is divided into the following periods: Hassunah, Halaf, Ubaid, Warka and Proto-Literate, Gaura and Ninevite ('Gaura', and 'Ninevite' apply to material found in the north of Mesopotamia, 'Warka', and 'Proto-Literate' to southern Mesopotamian fabrics). An immense amount of material is described, and discussed in full, from each of these divisions in turn, in a very clear and beautifully arranged manner. One can have nothing but praise for so complete a work. A good comment on the amount of material is the fact that a critic trying to find an omission had great difficulty, and only succeeded in tracing very small things, such as the omission of the motif of birds standing on each other's back in a vertical row, from the chart of Nineveh V designs. As a catalogue of material it is splendid, and the author deserves the highest praise.

The book is not intended as a historical work, but it is inevitable that some historical conclusions should appear. Some of them are most interesting. Miss Perkins defines two new periods, the Gaura Period, and the Ninevite Period. The former is defined as that time when the Tepe Gawra strata XI to VIII B inclusive were deposited and, at Nineveh, the Nineveh IV layers. The Ninevite Period is defined as the period of the Tepe Gawra VIII B and VII levels, and of the Nineveh V stratum. It is with regard to these, rather than the earlier periods, that Miss Perkins may find some opposition to her views.

The author dates the Warka Period as contemporary with the earlier half of the Gaura Period, and the Proto-Literate as contemporary with the later part of the Gaura Period and the Ninevite Period. Uruk IV she dates to the time of Gawra VIII, an unexceptionable conclusion. But she also dates the beginning of the Nineveh V Period to the time of Gawra VIII.

This book gives me the impression that a great stumbling block is the date of the Nineveh V Period, which I think she (like others) dates too early. She is aware of the difficulty, for, while agreeing that the objects probably 'looted' from the vaulted tombs built on a level below the top of the Nineveh IV deposits include amulets paralleled in Early Dynastic II contexts, she says that the tombs are likely to be considerably later than the bevelled-rim bowls, which occur only in Nineveh IV strata (with one at the top of Nineveh III). Yet there is plenty of Nineveh V pottery stratified on top of those beads and amulets 'looted' from the tombs (*L.A.A.A.*, Vol. XVIII, Plate 33), and amongst the 'loot' was a figurine of a Sumerian. I cannot see how one could argue that the vaulted tombs are any later, let alone much later, than the Nineveh IV Period. In that case, the Nineveh V Period must be later than the Early Dynastic Period. The amulets from the 'loot' from those tombs are paralleled in Tepe Gawra VI strata, in which deposit there is also ceramic decoration known in Nineveh IV strata, in the H and G strata at Ashur, and in the 'Royal Cemetery' at Ur (Plate LXX of the Gawra publication).

May we hope that, with her gift of real lucidity, Miss Perkins will explain these points, and increase our debt to her?

T. BURTON BROWN

**The Malays: A Cultural History.** By Sir Richard Winstedt. London (Routledge & Kegan Paul), 1950. Pp. vii, 198 and 8 plates. Price 15s.

This is a revised and enlarged edition of a work originally published in Singapore in 1947. It consists of chapters on Malay origin, migrations and language; beliefs and religion; social, political, legal and economic systems; literature; arts and crafts; and the future; with appendices of Malay texts and on relationships in Negri Sembilan.

It is an interesting and useful collection of material from scattered articles (many by the author) and other sources which are probably unfamiliar to readers who have not specialized in the study of this part of the world. The chapters on legal and social systems will probably be of most interest to anthropologists, but are—possibly through lack of reliable information—not as detailed as one might wish.

Unfortunately, the book is considerably marred by confusing multi-definitions of 'Malay,' by tenuous ethnological speculations and incautious generalizations, and by an apparent lack of acquaintance with any but the older schools of anthropology (an impression strengthened by the author's reference to 'the cloistered anthropologist' in contrast with the 'busy' magistrate or land officer). These easily discernible faults do not, however, distort the valuable parts of the text; the known facts from the history of the subjects dealt with. In general, it is a convenient first introduction of a general nature to an area which has so far had little attention from British social anthropologists.

RODNEY NEEDHAM

**Vanished Trails.** By R. L. Spittel. O.U.P., 1950. Pp. xvi, 259. Price 12s. 6d.

The subtitle explains that this work deals with the last of the Veddahs. Dr. Spittel is to be congratulated on accomplishing the seemingly impossible—the production, in the form of a novel, of a didactic account of the closing episode in the anthropological history of the last (from the cultural standpoint) of the jungle Veddahs. Those who are familiar with Spittel's earlier writings dealing with his jungle reminiscences and incorporating gems from his rich store of experience with the wild men—especially his *Savage Sanctuary* (1941)—will not be disappointed in this new contribution. It would have been unfortunate indeed if Dr. Spittel had not been blessed with a period of restful retirement wherein to set down for posterity his valuable gleanings, gathered during the busy life of surgeon over the last 40 years.

In the narrative the gradual transition is traced from the primitive cave-dwelling, food-gathering phase—still the regular custom at the dawn of this century—to the crude beginnings of the wattle-and-daub hut dweller and food-producer. The terrible hazards of a jungle existence are grimly and enthrallingly portrayed and feelingly illustrated by the delightful, yet thoroughly simple, black-and-white pen pictures by Gordon Davey.

This is a book to be read and re-read. There is a very useful introduction which gives us the essential anthropological and historical background for the narrative, and a glossary explaining many local terms, Veddah names of animals, plants and other features, without which it would be difficult for a reader unacquainted with the locale to understand the story.

W. C. OSMAN HILL

## EUROPE

**Anglo-Saxon Jewellery.** By Ronald Jessup. London (Faber), 1950. Pp. 148, 44 plates. Price £2 2s.

Anglo-Saxon Jewellery has an introduction of such length and importance that it should be considered as the first half of the book, the second half consisting of 40 pages of illustrations, preceded by a section of descriptive notes on each object represented. At the beginning of his introduction, Mr. Jessup explains that he is there mainly concerned with the progress of the ordinary reader. He has, indeed, marshalled all the information necessary to give the ordinary reader a picture of the kind of people the Anglo-Saxons were, where they came from, how they dressed and how they lived. Even the social status of the jeweller himself is

enquired into, before the materials, techniques and processes he used are examined. Particularly imaginative is the description of the exploits of the earlier antiquaries in their quest for collectors' pieces, which, until the Sutton Hoo find, formed the main body of the finer Anglo-Saxon jewellery.

The plates numbered IX to XL, together with the four coloured plates, form a useful collection of photographs of a variety of pieces of jewellery. Many of the illustrations are of normal size, but some, e.g. the Kentish jewelled brooches, are enlarged so that the details of the craftsman's technique are easily discernible. But the attractive qualities of many of the objects could have been made much more intriguing, had the opportunity been taken to make fresh



photographs with special attention to angles and lighting, and a glance at recent efforts in this direction (such as *Die Kunst der Germanen im frühen Mittelalter*, by W. A. von Jenny) will show how much more can be achieved. While the accompanying descriptive notes are sufficiently detailed to be clear, they do not succumb to the fate of severely archaeological descriptions, which, of necessity, often become rather dull reading. The serious student of Anglo-Saxon archaeology, however, will not find very much with which he is not already familiar, for, the more important pieces of jewellery, such as the Alfred Jewel and the Kingston brooch, have been justly famed for many years for their attractiveness, intrinsic value or excellence of craftsmanship. Nevertheless, it might perhaps have been advantageous on some counts to have dispensed with some of the illustrations of the well-known type of Kentish garnet cloisonné brooch, so that a greater variety of objects might have been included, e.g. belt mounts, strap ends, pendant jewels and small-long brooches, many of which show a high standard of workmanship.

Although Mr. Jessup states that it is not his intention to deal fully with Christian jewellery, one cannot help regretting the absence of a photograph of one of the circular brooches in the Trewiddle style, e.g. Beeston Tor, as a representative of the later accomplishments of the Anglo-Saxon in the reserved silver and niello technique. Indeed, the selection of objects for illustration seems at times somewhat arbitrary, as, for example, the inclusion of an Italian brooch (Plate XXXII, 2) and a Frankish buckle (Plate XXXVII, 2).

Controversial problems of Anglo-Saxon archaeology are not entered into, although references are given so that the reader may pursue them if he wishes. Nor is there any attempt at geographical or chronological grouping, with the result that there is no immediately obvious order in the plates, and it is startling to find the tenth-century Dowgate Hill brooch cheek by jowl with the seventh-century Forest Gate bead.

Such points, however, are scarcely likely to disturb a reader meeting Dark Age jewellery for the first time; for this is the first book by an archaeologist on Anglo-Saxon archaeology intended primarily to interest the many and not to inform the select few.

VERA I. EVISON

**Olddanske Tekstiler.** By Margrethe Hald. *Nordiske Fortidsminder*, Vol. V. Copenhagen, 1950. Pp. 496 (including English summary of 80 pp.), 450 illus.

**226** The latest work by Margrethe Hald is an impressive volume on ancient Danish textiles in which the scope is so wide and the mass of detailed information so great that this account can only touch the fringe of it.

The first part of the book, which is extremely interesting throughout, deals with the garments and other textile fragments found in the peat bogs, ancient settlements and graves of Denmark, which form the basis of the work. The positions of the finds are indicated on two maps and the skin and cloth garments, shoes, caps, bags, textile fragments, loom weights and other accessories, dating from 200 B.C. to A.D. 1400 are all described in detail and superbly illustrated by fine photographs and excellent diagrams. In a later chapter the shape of the garments, the relation between skin and cloth types and variations due to a possible change in the climate of the country are dealt with.

As the grave finds appear to fall between the Early Bronze Age and the birth of Christ much thought has been given to the dating of the specimens. Where objects of a known period have been found with them they have been helpful, but variations in spinning, the plain and different twill weaves, technical peculiarities, the use of tablet-woven headings, fringes, tubular edges, plaited-warp-loop borders, as well as, in the case of garments, the shape, have all been carefully considered and the tabulated results used for comparison. In filling in the gap in the grave finds material from the four following groups has been useful.

Huldermose, with a comb from the late Bronze Age or the beginning of the Iron Age, had two skin capes, an S-spun chequered kerchief with warp fringe and a tubular border, a chequered skirt, cloth with a plaited border of warp loops and other fragments. At the Borremose settlement a piece of four-shaft twill, with both warp and weft S-spun, was found in the moat of the fortification,

which is referred to the Celtic Iron Age, about 200 B.C.; other fragments also of four-shaft twill with S-spun yarn were found in Aars Parish, Borremose. At Corselitze a fibula dating from A.D. 400 was found in conjunction with plain and twill weaves of much finer texture having both warp and weft Z-spun; tablet-woven borders were also found. The Thorsbjerg find, which included material extending from the second to the fifth centuries A.D., had a piece of cloth with S-spun warp and weft and a tablet-woven border.

That two types of looms were used is practically certain, the upright warp-weighted, which probably passed through slight modifications, and a two-beam loom, fixed to uprights or some form of framework on which tubular weaving as well as plain cloth and four-selvaged cloth squares could be produced.

Some of the most interesting and unusual finds from the later periods came from a sepulchral chamber or stove-built coffin in Mammen Parish and Ronbjerg Mose. The latter had fragments of garments from the Middle Ages and in particular a cape or poncho in which the neck opening was made by turning back the weft on each side of it and greater width was provided at the shoulders by introducing extra warps. The rich collection of fragments from Mammen included part of a large garment or blanket embroidered in stem stitch with men's masks, animals, birds and acanthus, bands of silk with a tablet-woven border embroidered in gold thread, and portions of a padded silk fillet having a tablet-woven band and streamers of silk with insertions of looped needle netting. This beautiful and unusual work is ascribed to the Viking Age, but both earlier and later specimens are known and it still survives in Scandinavian countries. An example of double cloth weaving in silk was also found and called 'polymita.'

Special chapters, with detailed descriptions and plentiful working drawings, are allotted to braiding, cording, 'sprang,' tablet weaving and looped needle netting. The last named needs no other tool than a needle and expert fingers. 'Sprang' is an old and widely spread technique (Coptic examples are well known). It is worked on a rectangular frame by twisting the warp threads over one another to produce either a lace-like or a solid fabric. The twisting is done from both ends of the frame.

Margrethe Hald concludes her fascinating volume with an account of the geographical diffusion of some of the garments and textile techniques and their cultural relationship.

In conclusion it should be emphasized that the plentiful and superb photographs, the explanatory drawings and the summary make the valuable information in *Olddanske Tekstiler* available to all English-speaking readers.

LAURA E. START

**Armorica et Bretons.** By P. Giot. Rennes (University Press), 1951. Pp. 160. Price 500 francs

**227** The Armoricans are here the prehistoric peoples of Brittany, the Bretons the late Roman and subsequent immigrants. Skeletons are mainly from Morbihan and south-west Finistère. Survivors of late palaeolithic types are recognized (*archéomorphes*); early megalithic immigrants were moderately long-headed and a little over average height (1684 millimetres for 16 cases). Later there spread short broadheads from France and they reached the coast near St. Brieuc but affected Finistère relatively little. Collignon at the end of the last century thought the coast from St. Brieuc northwestwards had specially broadheaded men. Giot has organized measurements of conscripts and, for them, gives mainly cantonal averages, along with histograms which help to give a more detailed impression. On the whole measurements of the living seem to the author to confirm results of the study of buried skeletons. Pigmentation of hair and eyes receives little attention in these studies.

H. J. FLEURE

**Urgeschichte der Schweiz, Vol. I.** Edited by Otto Tschumi. Frauenfeld (Huber), 1949. Pp. xiv, 751

**228** This *Prehistory of Switzerland* not only brings together in one volume the rich Stone Age material of one small country but presents a textbook of the subject of great value to students of all regions and periods of European prehistory. For those



interested in the earlier Stone Ages and the environmental approach it is a very treasury of information.

The first section, by Heinz Bächler, is on the geological background, especially of the Alpine area, and on the climatic changes during the Pleistocene. The multiple subdivision of Penck's four glaciations is here accepted as a matter of course.

Walther Rytz sets forth the botanical environment, with full lists of fossil plant species for each site. An entire section deals with the theory, practice and results of pollen-analysis. The complete flora, both cultivated and wild, associated with the Neolithic and Bronze Age sites is followed by a discussion of the origins of cultivated plants.

More than a quarter of the volume is jointly contributed by the late Karl Hescheler and Emil Kuhn, on faunas from Tertiary times to the Iron Age. Many famous deposits are mentioned. Special attention is given to faunas as indices of human environment, and not only the mammals are considered. There are detailed lists from all important Swiss sites and a bibliography of 24 pages.

The human remains occupy the next section, by Otto Schläglhaufen. The poverty of Palaeolithic and Mesolithic fossils is disappointing to the human palaeontologist, but some too Neolithic individuals are known.

Otto Tschumi contributes the greater part of the volume, dealing with the Stone Age cultures, save for an opening section by Bächler on the high caves of Wildkirchli, Drachenloch and Wildenmannsloch. The history of the investigations, description of the caves, their sections and the faunal and cultural finds are very full. In succeeding sections Tschumi describes the caves of the Simmental, St. Brais and elsewhere and, finally, Cotencher.

There is an introductory summary on the Upper Palaeolithic of Europe. Only two distinguishable stages are found in Switzerland:

Gravettian and Magdalenian. The sites and finds are fully described.

The chapter on the Mesolithic begins with the progress of research from the days of the 'hiatus' to the present. There is a full account of the comparatively few Mesolithic discoveries in Switzerland and a reconstruction of the food-gathering way of life based on the archaeological material.

With the Neolithic, the Swiss archaeological record becomes one of the richest and most important in Europe. The pile dwellings and other settlements, the evidences for hunting, fishing, agriculture, stock-keeping and dairying and the wealth of perishable material preserved are fully illustrated. Tools, handicrafts and their products are discussed from the technical standpoint.

Under the heading 'spiritual culture' come burial rites and grave goods, megalithic monuments and their distribution. The impact of the bell-beaker culture on the region is described.

A tentative arrangement in three chronologically successive groups of cultures is supported on available stratigraphical data, with the principal 'zone fossils' indicated in each case. Even linguistic and philological evidence of folk movements is considered.

The last section deals with magico-religious manifestations from Palaeolithic cave art to surviving Swiss customs, the latter probably originating in prehistoric agricultural fertility magic.

An appendix maps and briefly describes the chief lake-village sites.

Only a number of authors could have written on subjects so widely different yet all so germane to the general theme. Only a panel of reviewers of equivalent expertise could be expected to deliver at length a balanced judgment on the whole. It is certain that most readers, like myself, will be tempted away from their own special interests into fields much less familiar, to their very great profit.

I. W. CORNWALL

## CORRESPONDENCE

**Race.** Cf. especially MAN, 1951, 95 and 96

**229** SIR,—Dr. Trevor's distinctions in anthropology, regrettably unknown outside his immediate circle, seem to have gained him the privilege of adding a note to my recent letter.<sup>1</sup> The opportunity might have been gainfully employed, but he has preferred to put it to the satisfaction of his own ego and the entertainment of his friends. They might be amused by his heavy wit, agree with his imputations, and be impressed by his peculiarly unselective references; but I gather that there are others who will fail to share these pleasures. I leave him to them. Dr. Don J. Hager has, in fact, independently answered most of his letter.

Meanwhile, I must continue to prefer the opinions of great classical scholars, such as Fowler and De Slane, on the philology of the words 'race' and 'mulatto,' especially as my confidence in Trevor's Latin is not enhanced by his guesses. On taxonomy I must also beg to prefer taxonomists to Trevor, while on Dunbar I fancy that Laing, Schipper, Craigie, Baildon and MacKenzie might be better informed than a student of the quotations in the O.E.D. Laing, it is true, gives 'and bak-byttaris of sindry racis' as an alternative reading according to the Maitland MS., but Dr. Craigie (co-editor of the O.E.D. and the greatest authority on Dunbar) supports 'and bak-byttaris in secrete plices' with a clear photograph of the relevant portion of the Maitland MS.<sup>2</sup> Moreover, if Trevor had a larger familiarity with the history of racialism and the word 'race,'<sup>3</sup> he might have saved himself this unfortunate excursion into an unfamiliar field. But in that case he might have deprived himself altogether of the relief of undressing himself in public—and I hesitate to suggest that the austerities of MAN should not sometimes be relaxed.

London

CEDRIC DOVER

<sup>1</sup> MAN, 1951, 95

<sup>2</sup> W. A. Craigie, *The Maitland Folio Manuscript*, Edinburgh and

London (Scottish Text Society & Blackwood), 1919, Vol. I, p. 14 and facing plate.

<sup>3</sup> I should be grateful for any reference to the use of the word 'race,' in a larger ethnic sense than that of 'breed,' before 1575, when George Gascoigne wrote in *The Posies*:

'This Apuleius was in Affricke borne,  
And tooke delight to travaille Thessaly,  
As one that helde his native soyle in skorne,  
In foraine coastes to feede his fantasie . . .  
His worthie race he (recklesse) doth forget . . .'

### The Last Years of Ahuia Ova

**230** SIR,—Ahuia Ova will always be remembered by anthropologists because of his close association with two of the most brilliant figures in Papuan ethnography—C. G. Seligman and F. E. Williams. It was Ahuia Ova who gave Seligman the bulk of his material for the Koita section of *The Melanesians of British New Guinea* (Cambridge, 1910). Although Williams took many notes of Hanuabada (Ahuia's village near Port Moresby), the only ones published are his attempts to reconstruct Ahuia's story as a text in Papuan history, attempts which he found disappointing ('The Reminiscences of Ahuia Ova,' *J. Roy. Anthropol. Inst.*, Vol. LXIX, 1939).

Ahuia Ova is still alive (fig. 1), living in semi-exile in the Koita village of Kila Kila, some miles to the east of Port Moresby. His story over the past few years illustrates the decline of a man who has lived beyond his prime and as a consequence lost the political power that was so dear to him.

We may take up his story in 1942, when the Japanese were beginning their advance on Port Moresby, a town converted into an encampment. Ahuia was then still leader of the Koita section of Hanuabada, paramount though not unchallenged, and his influence even extended over the Motu section. Pressing military needs led to the evacuation of the village to various points along the west coast,





FIG. 1. AHUIA OVA, NOVEMBER, 1950

*Photographs by C. S. Belshaw*

but Ahuia moved to Yule Island, where there is a Roman Catholic Mission, and where he did some Bible translation work. He returned to his people after some months, but was very soon forced to spend some time in hospital.

Ahuia had no children of his own, but he had adopted the son of his wife's brother. This man had two daughters, who had accompanied Ahuia to Port Moresby when he was sick, and who resisted his return to the Koita village then being built near Porebada. One of these girls, Goka (Margaret) Oala, who had followed Ahuia into the Catholic faith, fell in love with a half-caste who was a member of the Church of England. When the people returned from evacuation after the war, the half-caste asked Ahuia for Margaret's hand in marriage.

Ahuia blames himself bitterly for what followed. 'I was so silly myself,' he said, 'I didn't think.' For he decided that the half-caste was a suitable, hard-working husband—and overlooked the other question of marriage outside the faith. At first all was well. The Catholic authorities, though censuring Ahuia, decided that matters had gone too far, and that the marriage would have to proceed. But then Margaret had a miscarriage and would have lost her life but for a blood transfusion from her husband. A second child was born in hospital with pain and difficulty. Ahuia, attempting to atone for his previous error, called the Catholic authorities. The Sisters made the child a little dress and took it for baptism. But here the husband stepped in and prevented it. In two weeks' time the mother died, and then the child. Ahuia Ova believes, with a resignation that is painful to see, that this tragedy was his own doing, a visitation for his sins.

The fall from political power is a longer and rather typical story. As the result of the inheritance of important lands, of his close friendship with European authorities, and of his own driving and intelligent personality, Ahuia was certainly the most powerful man in Hohodae (the Koita section of Hanuabada) from 1900 to 1940; and his position in the village was rivalled only by that of Peter Vagi and Gavera Arua, both Motuans. According to Ahuia's story, which is quite frank and which I have checked in Hohodae, discontent with Ahuia's power began to come into the open about 1932 when there was an objection raised to the man Ahuia had appointed

as feast chief. At this time Ahuia offered to leave Hanuabada, but was restrained by the then Governor, the late Sir Hubert Murray.

But after the war the discontented elements could not be contained, and Ahuia, sick in body, blind in one eye, and struck by the death of his 'grand-daughter,' could not stand up to them. As excuse, they raised the question of Ahuia's inheritance of land. Ahuia's mother married a Kila Kila man, but quarrelled with him and went to live with her brother, Ova, at Hohodae. Here Ahuia was born, and Ova adopted him, gave him his name, and clearly promised him the inheritance, to which he duly succeeded, and whence he derived his power. But when the decline came, one of Ova's brother's sons, Eu Nagi Tau, accused Ahuia of usurping his rights, and took the case to the District Officer. The District Officer, and other Europeans, supported Ahuia, but by this time his cousins, on whom he depended for sustenance in his old age, had deserted



FIG. 2. AHUIA'S HOME AT KILA KILA

him. His position was untenable and on 13 November, 1948, he left Hohodae for Kila Kila (fig. 2).

Quite apart from his old age, there are many possible reasons for Ahuia's undoubted unpopularity. His religion set him apart, for the majority of his people are followers of the London Missionary Society; though this does not seem to have been held against him. But he depended too much on European authority to advance claims of leadership, not only over his patrilineal clan, but over the whole Koita section, and even over the Motu section too. He is an intelligent man, and was much favoured by Sir Hubert Murray, who almost exercised a personal administration over the village. But there are signs that Ahuia did not always use his authority traditionally or well, and that some of his decisions may have caused resentment which came into the open in his moment of weakness. A case in point is that of the man with whom he is now living. This man wished to marry one of Ahuia's relatives, but her parents objected and Ahuia himself assisted the elopement, the couple living with him for a while. 'I gave her myself,' said Ahuia, 'because I had power and respect.'

This story well illustrates some of the difficulties of adjustment which modern Papuans have to face; of interpreting rival religious dogmas, of living in a framework of sectarianism, of exercising authority with an added basis of European power. It illustrates, too, the way in which leaders may eventually be removed from office, by gentle but uncompromising social pressure. But it is a sad story to those who recognize Ahuia's contribution to anthropology, and who have met him, dignified, half-blind, and lonely, with not an interest in the world but the telling of stories.

*Port Moresby, Papua*

CYRIL S. BELSHAW

SIR.—Ahuia Ova, whose recent history I described in an earlier letter, died after a brief period of illness on 23 April, 1951. He was brought back to Hohodae to die in the house of his mother's brother's grandson, and after the usual wailing was buried in the Hanuabada cemetery. Some of his stories of clan origin and some of his extensive genealogical knowledge have been preserved in a large record book kept by the Hohodae people.

*Port Moresby, Papua*

CYRIL S. BELSHAW



## Royal Anthropological Institute

### CURL BEQUEST PRIZE

The Council of the Royal Anthropological Institute announces that it will (subject as mentioned in the Rules for the time being governing the competition for the above prize) again award in 1952 and thereafter every year the Curl Bequest Prize for the best essay by any competitor upon the results or analysis of all or any anthropological work carried out or published during the period of ten years preceding the year in which such essay is submitted and/or the history of some useful line in anthropology during that period.

Until further notice the rules governing the competition are:—

- (1) Essays shall be submitted not later than 30th April each year.
- (2) They shall be in typescript in English, French or German.
- (3) Essays shall be in literary form and not in the form of bibliographies or catalogues.
- (4) The length of an essay shall not exceed 25,000 words or be less than 10,000 words.
- (5) The decision of the Council of the Institute or of such officers of the Institute as the Council may from time to time appoint for the purpose of judging the respective scientific merits of the essays submitted shall be final as to the best essay and upon all other questions arising in connection with the essay competition.
- (6) If, in any year, there shall be no essay which, in the opinion of the Council of the Institute or of the officers of the Institute appointed for the purpose under the last preceding rule, is of sufficient scientific merit to deserve the award of the prize, then no award shall be made in that year. The amount of the prize available for that year shall be retained by the Institute and added to the prize in any later year, in which there shall be at least two essays which are adjudged of sufficient scientific merit to deserve the award of the prize.
- (7) If in any year there shall be two or more essays which are judged of equal merit and scientific value and worthy of the award of the prize, then the amount available for the prize in that year may be divided.
- (8) The winning essay or essays shall be read at the last meeting of the Institute in December or at the first meeting in January of the following year.
- (9) The winning essay or essays shall be published in the *Journal of the Royal Anthropological Institute* or, at the discretion of the Council, may be published under its direction in the same style as the other publications of the Institute, or in both these modes.

The prize offered for the winning essay in 1952 is £50. Intending competitors should forward their essays before 30th April, 1952, to the Hon. Secretary, Royal Anthropological Institute, 21, Bedford Square, London, W.C.1, to whom inquiries should also be addressed.



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(a) RAM-HEADED ÆGIS DUG UP AT APAPA,  
LAGOS

*Photograph by courtesy of the Trustees of the British Museum*



(b) RAM-HEADED ÆGIS ON THE PROW OF THE  
SACRED BARQUE OF AMON-RÊ  
*From a relief of Ramesses III*



(c) RAM-HEADED ÆGIS IN THE BENIN N.A. MUSEUM

*Photograph: William Fagg, 1950*



(d) RAM-HEADED ÆGIS IN THE BENIN N.A. MUSEUM

*Photograph: P.R.O. Nigeria, 1949*



# THE EGYPTIAN ORIGIN OF A RAM-HEADED BREASTPLATE FROM LAGOS\*

by

G. A. WAINWRIGHT, B.LITT.

**231** It is a commonplace that among the Negroes there are numerous survivals from ancient Egypt. Indeed, two very striking examples have recently been published by Arkell,<sup>1</sup> though in this case the two originals do not come from Pharaonic days or indeed from Egypt proper. On the contrary they belong to the X-Group civilization, coming from Firka in Nubia half-way between the Second and Third Cataracts of the Nile, and dating to the fifth or early sixth centuries A.D.<sup>2</sup> Knowledge of such originals reached the Gold Coast in due time, though unfortunately there is no evidence as to when it arrived or the date at which the two derivatives were made. They were dug up at Attabubu in Bron country north of Kumasi.

In the present article another instance of survival is studied. In this case the original is purely Pharaonic, though the intermediary would probably have been Meroë a little north of Khartoum. The West African survival this time is a priestly breastplate, and it comes not from Kumasi on the Gold Coast but from Lagos in Nigeria.

The breastplate (Plate Ja) is now in the Ethnographical Collections of the British Museum, No. 1930. 4-23. 1. My thanks are due to the Trustees for allowing the use again of the photograph published by Joyce in the *British Museum Quarterly*, Vol. V (1930-31), Plate xvii, and p. 30, No. 29. The breastplate is a large and massive object, being some 8½ inches (22 cms.) across the broadest part and 17½ inches (44.5 cms.) in overall length from the top of the hook to the bottom of the jingles.

Alongside the Nigerian breastplate is an ancient Egyptian picture of the head of the sacred ram of Amon-rê wearing the great collar called the *wesekh*, i.e. 'the broad' (Plate Jb). It is due to the Egyptian convention of drawing that the ram's head is shown in profile; in the object itself the head would have been facing the spectator just as it is in the Lagos specimen. In real life these Egyptian collars were made of beadwork and when worn by human beings they spread all across the chest and on to and even over the shoulders.<sup>3</sup> The present example is taken from a relief of Ramesses III, c. 1198-1167 B.C., where it is attached to the poop of the sacred barque of Amon-rê,<sup>4</sup> and on the real barque it would have been made of metal just like the one from Lagos. These *wesekh* collars are not peculiar to Amon-rê but are used by other deities, the best-known being the symbol of the cat-headed goddess Bast. In her case it was naturally the cat's head from which the *wesekh* collar depends, just as in Amon-rê's case it is the ram's head. The whole thing, cat's head and collar, is called by archaeologists 'the ægis of Bast,'<sup>5</sup> and so the Amûn symbol might be called 'the ægis of Amon-rê.' Similarly in West Africa these 'ægides' are not peculiar to the ram god, for Mrs. Meyerowitz publishes one showing a man

standing on catfish.<sup>6</sup> Like the 'ægis' of the ram god, this one shows its Egyptian inspiration, for the man carries a very definite crook of Osiris.

Amon-rê's worship was very strong in Ethiopia both at Napata and at Meroë, at the latter of which cities little 'ægides' of his were set upon finger rings<sup>7</sup> which date to about 45-15 B.C. Amon-rê's worship also spread out into Libya, where in classical times the Oasis of Jupiter Ammon (Siwah) was famous, and further west again figures of the ram with the sun between his horns are engraved on the rocks in both the Algerian coastland and the Sahara.<sup>8</sup>

The breastplate or 'ægis' from Lagos shows itself very clearly to be a derivative from Egypt. The head is that of the Amûn ram with horns curving downwards round the ears like the Egyptian original.<sup>9</sup> The plate is edged with plaited wire, a motif that is very common in Egyptian and Meroitic jewellery.<sup>10</sup> On the plate a band of floral decoration is engraved which is derived from the lotus flower of Egypt and forms the outer row of the Theban example. The plate is fringed with more flowers. These may have originated as a repetition of the lotuses bordering the Egyptian specimen or perhaps from those hanging from the pectoral or plaque in front of it, though their shape looks more like buds. On the other hand they look less unlike the little cowrie shells which dangle round the edge of such an 'ægis' as No. 166a of the Meroitic hoard of jewellery which has just been mentioned. The jingles, rattles or, as Joyce calls them, cascabels which dangle may also be remote derivatives from these hanging lotus flowers, for in another country this common Egyptian edging gave rise to bells. This was at the hem of Aaron's robe, which is described in Exodus xxviii, 33-35 as having 'a golden bell and a pomegranate' alternating with each other.<sup>11</sup> There were four little golden bells in the great find of jewellery at Meroë, which, as stated above, dates to a period between 45 and 15 B.C.<sup>12</sup> Little bells were common in the X-Group times in Nubia, c. A.D. 350-600, when they were attached to the harness of the horses, donkeys and camels. All of these, however, whether Meroitic or X-Group, were campaniform of one sort or another,<sup>13</sup> which the West African ones are not. These Lagos jingles are, however, quite like the buds which usually occupy the spaces between the open lotuses and which became the 'pomegranates' of Aaron's robe. More especially are they like the buds on Tutankhamûn's piece of jewellery which have the band round them and the divisions down the lower half.<sup>14</sup> Again, in being banded they are like the great roundish beads with their gold bands from the above-mentioned treasure from Meroë.<sup>15</sup> But yet again it may be that the mere action of time and space has changed the shape from that of bells to jingles.

There is another point that emphasizes the Egyptian origin of the breastplate from Lagos, and that is the floral

\*With Plate J



character of the decoration. While this is usual and common in Egyptian art, Mr. Penniman has kindly pointed out that it is not normal in African art, the motifs of which are animal, human or geometric forms.<sup>16</sup>

Mrs. Meyerowitz has published<sup>17</sup> another breastplate of the same type. It has the ram's head just as has the one from Lagos, but it came from Benin; and Fagg tells me that there is another at that city.<sup>18</sup> There are also two fragmentary ones in the British Museum. Actually the Lagos specimen also might have come from that city originally, for Fagg kindly tells me that its workmanship is very like that of the Bini. Moreover, the whole of what is now Lagos Colony seems to have been under the domination of Benin before the arrival of the first Portuguese explorer, Sequeira, in 1472. In fact one of the Obas of Benin seems to have stayed a considerable time in Lagos and to have made it his headquarters after his return from a visit to Portugal.<sup>19</sup>

Mrs. Meyerowitz assigns her Benin breastplate to Shango. He seems to be derived from the famous god of ancient Egypt, Amon-rê, for it is he who sends the rain and casts the thunderbolt, and according to her and Frobenius his sacred animal is the ram.<sup>20</sup>

Amûn was a sky god; his sacred object was almost certainly a meteorite,<sup>21</sup> and he was derived from the still older Min whose sacred object was the thunderbolt.<sup>22</sup> Later on Amûn became solarized as Amon-rê (Rê being the name of the Sun god), thus suffering the fate of most other Egyptian gods. His sacred animal was the ram. Just as his worship spread into Libya, so the idea that the ram is connected with thunder and storm has become widespread in West Africa. It exists in Calabar and the Cameroons<sup>23</sup> and in the same neighbourhood another tribe sacrifices a ram to the thunder god.<sup>24</sup> Elsewhere, the idea is found again in northern Togoland, also among the Mossi of the Upper Volta Province, and much further on again among the Mande in Senegambia.<sup>25</sup>

It has just been noted that the ram god of Egypt finally came to have a double nature. The original Amûn was a sky god but, as has been the way of old sky gods, he took on the character of a sun god and became solarized into Amon-rê. The association of the ram with the sky and storm in West Africa has been detailed above, and now it should be remarked that his other side, that of sun god, also appears in Africa. That is among the tribes along the Benue River, and in that they agree with the Hausa to the north of them.<sup>26</sup> The Hausa live on the southern edge of the Sahara, along the northern edge of which the solarization of the ram is so strongly marked in the *graffiti* mentioned earlier in this article.

However, the storm was not ignored in ancient Libya. It was told that, when Alexander the Great was on his way to Siwah in Cyrenaica to consult the oracle of Zeus Ammon, the god relieved him of thirst by sending a shower of rain.<sup>27</sup> Another story went that Zeus Ammon saved Dionysus' army from dying of thirst in the Libyan desert by sending a ram which pawed the ground, causing a spring of water to gush out.<sup>28</sup> The ram, it will be remembered, was the god's sacred animal. Also in Cyrenaica, and presumably at Siwah like the oracle, there was

a sacred rock which, if desecrated, caused a violent storm of sand to arise.<sup>29</sup> Thus, then, at ancient Siwah Zeus Ammon and his ram were much concerned with storms, whether of rain or sand, and with the production of water.

There was a great road, along which travelled relics of Egyptian or Meroitic civilization. From Ethiopia this road ran all across Africa on the southern side of the Sahara. It went *via* the south of Lake Chad and so on out westwards even as far as Senegambia. On the southern side of this country the Benue proves to have been a great highway for Egyptian influences, as I have recently shown in an article in the *Journal of Egyptian Archaeology*, Vol. XXXV, pp. 170-5.

#### Notes

<sup>1</sup> *Antiquity*, 1950, pp. 38-40 and plate facing p. 38.

<sup>2</sup> L. P. Kirwan, *The Oxford University Excavations at Firkia*, pp. 30f.

<sup>3</sup> They are common enough in pictures and sculptures; Petrie and Brunton, *Sedment*, Vol. I, Plate XVIII, *Sedment*, Vol. II, Plate LV, 21; Howard Carter, *The Tomb of Tut-ankh-Amen*, Vol. II, Plate LXXI; Vol. III, Frontispiece. For some real ones see E. Vernier, *Bijoux et orfèvreries*, Plates XL, LII, LXVI, CIII (*Cat. gén. des antiq. ég. du Musée du Caire*); Naville, Hall and Ayrton, *The Eleventh Dynasty Temple at Deir el Bahari*, Vol. I, Plate X, top. A real one made of beads, berries, flowers, etc., was found round the neck of one of Tutankhamûn's coffins, Carter, *op. cit.*, Vol. II, Plate XXXVI, and pp. 191f., no. iii.

<sup>4</sup> H. H. Nelson and others, *Medinet Habu*, Vol. IV, *Festival Scenes of Ramses III*, Plate 229. These 'ægides' were also set upon staves (A. M. Calverley and others, *The Temple of King Sethos I at Abydos*, Vol. II, Plate XI, bottom; Boreux in *J. Egypt. Arch.*, Vol. VII, Plate XIX and pp. 113-20). They were evidently to be carried in procession (Gardiner in *J.E.A.*, Vol. XXXIV, pp. 21f.). For these staves in general see Spiegelberg in *Rec. de Trav. relatifs à la philol. et à l'arch. égyptiennes et assyriennes* (Paris), Vol. XXV, pp. 184ff.

<sup>5</sup> G. Daressy, *Statues de divinités*, Plate L, top row (*Cat. gén. des antiq. ég. du Musée du Caire*). For a pair of small gold ones to be worn as amulets see Vernier, *op. cit.*, Plate LII, top.

<sup>6</sup> Meyerowitz in *MAN*, 1940, 155, Plate I-J, fig. 1. She says the man represents the god Olokun, though Fagg tells me that according to local tradition he is an early Oba.

<sup>7</sup> H. Schäfer, G. Möller and W. Schubart, *Ägyptische Goldschmiedearbeiten*, Plate XXII, Nos. 162, 163, 164, 165, and pp. 113-19. On p. 112 fig. 112 shows no. 165 worn on a living hand.

Fagg has very kindly pointed out to me the head, probably from Benin, which was lent by Mr. W. O. Oldman to the exhibition of Traditional Art of the British Colonies held at the Royal Anthropological Institute in 1949. It was No. 25 and appears in Plate 9 of the Catalogue. It is adorned with three ram ægides represented as hanging from the hair and they have pendants dangling from them very like those on the Meroitic finger rings. The head is now in the British Museum.

It perhaps seems a curious way in which to wear such things, so it is probably worth while recalling the X-Group crowns from Ballana in Nubia, which date to the period c. A.D. 350-600, especially in view of the very obvious West African survivals from this period or shortly afterwards which have already been mentioned in the first paragraph of this article.

At Ballana the ægides are arranged in a row all round the crown, generally alternating with gem stones. These ægides, however, are human-headed (Emery and Kirwan, *The Royal Tombs of Ballana and Qustul*, Plate 32, nos. B95/18, 22; Plate 34, nos. B114/17, B6/20; Plate 35, no. B47/13). But still, on top of two of the crowns, the head of Amûn's ram appears large, crowned with plumes, etc., as the centre piece of the whole, as it does on another (*ibid.*, Plate 33, no. B80/48). The wearing of ram ægides on the head from Benin may perhaps result from a dim memory of the ornaments on the royal crowns of Nubia confused with one of ægides worn as personal jewellery.



<sup>8</sup> Frobenius, *The Voice of Africa*, Vol. I, p. 225; O. Bates, *The Eastern Libyans*, pp. 195-200. Joleaud in *J. soc. des africanistes*, Vol. III (1933), pp. 197-282, has an important study of these Saharan graffiti, of the significance of the ram, etc., and on Plate II, figs. 4, 5, 6, shows some fine examples of these rams in photograph, and facing p. 221, fig. 3, shows others in outline drawing.

<sup>9</sup> Joyce suggested that it might be that of a gnu, *loc. cit.*

<sup>10</sup> For Egypt, Vernier, *op. cit.*, Plates X, XVIII, etc.; for Meroë, Schäfer, Möller and Schubart, *op. cit.*, Plates 21-23 often.

<sup>11</sup> Petrie, *Egypt and Israel*, pp. 61, 62 and fig. 31. For more of these lotus edgings see Petrie, *Egyptian Decorative Art*, figs. 120-26. For two actual pieces of jewellery with the lotus and bud pendants see Howard Carter, *op. cit.*, Vol. III, Plate XIX, fig. B. The motif between the open lotus flowers varies considerably, from buds to seed pods, bunches of grapes, etc., even to small open lotuses.

<sup>12</sup> Schäfer, Möller and Schubart, *op. cit.*, Plate 32, no. 278 and p. 167=C. R. Lepsius, *Denkmäler aus Aegypten und Aethiopien*, Vol. V, Plate 42, nos. 28-31 in colour. For a study of the date see Wainwright in *Sudan Notes and Records*, Vol. XXVI (1945), p. 25.

<sup>13</sup> Emery and Kirwan, *op. cit.*, pp. 263-71 and fig. 94. The variety of the shapes is great. H. Hickmann, *Instrumente de musique* (*Cat. gén. des antiq. égypt. du Musée du Caire*), Plates XXIII-XXXVI, where many of those from Ballana and Qustul are shown among the others. Plate XXIX A and B shows how they were worn by the horses.

<sup>14</sup> H. Carter, *loc. cit.*

<sup>15</sup> Schäfer, Möller and Schubart, *op. cit.*, Plate 32, figs. 272, a, b, and p. 164.

<sup>16</sup> On some of the Benin plaques a diaper pattern and trefoils are used as a background which look as if they were derived from flowers or leaves. Others of the plaques show trees as part of scenes. But these are exceptional and are no doubt due to Portuguese influence.

<sup>17</sup> Meyerowitz in *MAN*, 1940, 155 and Plate I-J, fig. 3. These bronzes have been thought to have been of Ife origin, but Fagg does not feel convinced of this. He thinks they may have a connexion with certain bronzes which belonged to Tsoede, the fifteenth-century chief of Idah (*MAN*, 1950, 98).

<sup>18</sup> New photographs of the two ram-head agides at Benin are reproduced in Plate Jc, d; that shown in (c) has a border design identical with that of the Lagos piece (a). Ram worship seems to have had some importance at Benin, for apart from the two pectorals, this city has produced a large bronze figure of a ram as well. It is shown as No. 26 in Plate 10 of the above-mentioned catalogue and is now in the British Museum. From Owo about 60 miles north of Benin

there come a number of wood carvings of rams' heads and of human heads wearing rams' horns; Fagg publishes one of each of them in *MAN*, 1951, 124, Plate Fb, c. Elsewhere, at Abiri not far from Ife, 100 miles or more north-west of Benin, there has been found an ancient ram's head in terra cotta. It seems to represent the head of a sacrifice on a platter (Fagg in *Image*, No. 2, 1949, p. 28, and reprinted in the *American Magazine of Art*, 1950, p. 133. It is mentioned in the R.A.I. Exhibition Catalogue, p. 2, no. 5).

<sup>19</sup> P. A. Talbot, *The Peoples of Southern Nigeria*, Vol. I, pp. 79f.

<sup>20</sup> Meyerowitz in *MAN*, 1946, 27; Frobenius, Vol. I, pp. 205, 210, 212, 217f.

<sup>21</sup> For Amûn as a sky god see Wainwright in *J.E.A.*, Vol. XX, pp. 139-53. For the meteorite as his sacred object see *id.* in *Annales du service des antiquités de l'Égypte* (Cairo), Vol. XXVIII, pp. 183-7, Vol. XLII, pp. 183-5; *J.E.A.*, Vol. XVI, pp. 35-8; *Zeits. f. äg. Sprache und Altertumskunde*, Vol. LXXI, pp. 41-4.

<sup>22</sup> For Min's thunderbolt see Wainwright in *J.E.A.*, Vol. XVII, pp. 185-95. For the derivation of Amûn from Min see *id.* in *J.E.A.*, Vol. XX, pp. 139f.

<sup>23</sup> P. A. Talbot, *op. cit.*, Vol. III, p. 969.

<sup>24</sup> *Id.*, *Tribes of the Niger Delta*, p. 24.

<sup>25</sup> Frobenius, *op. cit.*, Vol. I, pp. 219-21.

<sup>26</sup> *Ibid.*, Vol. I, p. 221.

<sup>27</sup> Arrian, *Anabasis*, III, 3, §4; Quintus Curtius Rufus, IV, Ch. vii, 30.

<sup>28</sup> O. Bates, *The Eastern Libyans*, p. 189, gives a full list of references to this story. In Egypt ram gods were concerned with the outpourings of the earthly waters at the First Cataract where the river surges into Egypt and at Heracleopolis where the Nile pours into the Fayyum. Amûn and his ram were concerned with the earthly waters. In fact this ram was called *Rhny*, 'The Wader in a Lake,' and a picture of the ram's head is entitled 'Amûn of the Lake' (Wainwright in *J.E.A.*, Vol. XX, pp. 141-3).

<sup>29</sup> Pomponius Mela, I, 8; 'a certain rock (*rupes*) sacred to the south wind. When it is touched by the hand of man, that violent wind arises and, driving the sands like seas, rages as if with waves.' Pliny, *Nat. Hist.*, II, 45, explains the touching as sacrilege. This is on the northern side of the Sahara. On the southern side it is thought at Haraza in northern Kordofan that if ancient iron implements be left lying about on the ground, they will attract the north wind and the corn will be overwhelmed with sand (H. A. MacMichael, *The Tribes of Northern and Central Kordofan*, p. 91, note 3). Iron is of course a meteoric (thunderbolt) material and commonly sacred to storm gods, so that in each case here the sandstorm takes the place of the rainstorm.

## THE CULTURAL PROCESS IN INDIA

by

DR. IRAWATI KARVE

Poona

**232** The cultural process in India is one in which different communities live side by side, interchanging ideas and goods but not blood. In certain areas the juxtaposed communities are more or less independent of one another, in others they are economically interdependent. The anthropological evidence shows that in each cultural region there are distinct racial strata which can be correlated to social strata and that racial intermixture, though found in all regions, is not very great in depth and extent. Neither is one racial element always the one to be socially supreme, the social position of different race groups changing from region to region.

North-western Orissa is inhabited by at least a dozen

different communities. In the Padampur area the mongoloid Binzals cultivate paddy and the ruling house also belongs to this community; in the same town live the non-mongoloid Mehras or weavers. The Binzals are big-headed with broad faces; the Mehras' heads are definitely smaller, the faces longer and noses finer. All Binzals buy their clothes from Mehras; all Mehras buy their rice from the Binzals; both observe certain common festivals, but there is not a single record of intermarriage. The Saoras (semi-primitive hunters), the Keuts (fishermen) and the Kultas (cultivators) live in an adjacent region in the same way. In Sundergad the Mundas, Oraons, Kisans and Agarias are all cultivators, but each group lives separated



from the others; intermarriage is not even thought of. This separateness is not a feature of primitive and backward regions only. The Kadwa Kanabi and the Rabari of north Gujarat live together in just the same way: the Kanabi is a skilful cultivator and cultivates varied crops like wheat, millet, cumiss, coriander and aniseed; the Rabaris are purely pastoral, keeping mixed stocks of cows, buffaloes, goats, sheep and camels. Racially they do not appear to be as different from each other as are the peoples of north-western Orissa. The Delhi region is settled by Jats, Gujars and Meos: the Jats hold the best lands and are deemed to be the most skilful agriculturists; Gujars occupy less favourable country and the Meos are Muslims of mixed origin, occupying a tract different from the other two groups. These examples can be multiplied for other regions, and the more one studies Indian social institutions, the more convinced one becomes that the caste system is a result of this peculiar cultural process by which people of different stocks or people of the same stock coming into a region at different times lived juxtaposed without mixing. The caste system is supposed to have divided Hindu society into segments and introduced an element of disunity. This description of the social effect of such a system may be right, but from a historical point of view it is topsy-turvy. It was the primary mode of acculturation which evolved a caste system and gave the loosely interdependent folk elements in a region a basis of organization. It was not a creation of the Aryans or the Brahmans because in the *Mahabharata*, which describes the social life of the ruling Aryans, the caste-like juxtaposition of the other racial elements is already evident. The treatment which the topic of caste receives from Smritikars is so ridiculous that it is evident that they were dealing with a phenomenon foreign to the original Vedic-Aryan peoples.

The ways of racial mixture within such groups have been determined since very ancient times—probably long before the Vedic people arrived. The racial mixture could take place through concubinage or hypergamy. A ruling class or a rich man could have maids from a 'lower' class, the conquered folk elements. Again a ruling class could take wives from among the daughters of rival ethnic elements, considered to be on a slightly lower level. Both these ways have been institutionalized, but they allow of mixture to a very small degree and in well defined channels. Hypergamy in its turn has given rise to a preferential type of cross-cousin marriage, that in which a man marries his maternal uncle's daughter but never his paternal aunt's daughter. Significantly enough, this mode is the most prevalent in the cultural contact zone from the Vindhyan region to the southern Maratha country.

This mode of corporate life must have existed before the arrival of both the Aryans and Dravidians, and the later post-Christian immigrants must have simply followed an already well set pattern. Until the arrival of the Muslims there was no change. Conversion and abduction of women upset this pattern to some extent, but it asserted itself again in the continuation of the caste system among Muslims and Christians, converts and non-converts.

There was a continuous give and take on an ideational

plane. The borrowing was not only one-sided. Neighbours borrowed social institutions and deities from one another. The clan system of the pre-Aryans was borrowed to a great extent by the Vedic Brahmans. Marriage in ancient times had but one taboo, that of consanguinity. Later on the Gotra system forbade marriage among people of the same Gotra. Thus Gotra was modelled on exogamous clans. The non-Vedic people in their turn borrowed the idea of Gotra and we find many people adopting fantastic Gotras, or calling their clans Gotras: the ruling house of Binzals have a Rameshwar Gotra; and the Oraons and Mundas call their clans Gotras. On the other hand the Brahmans in Orissa have associated their Gotra names with different kinds of birds, e.g. the Brahmans of Bhardwaja gotra hold the blue jay (*bhadrabadalia*) as sacred, the Brahmans of Harit gotra hold the golden oriole (*haladi basanta*) as sacred, and the Brahmans of Atri gotra will never eat the meat of a deer, though the meat of a black buck or a stag is not taboo. In the Maratha country, the Brahmans, besides Gotras, possess surnames which are those of animals, vegetables and inanimate objects, e.g. tiger, horse, sheep, cow, wolf, ladies' fingers, cucumbers, or agricultural implements like axe, pick-axe, shovel, etc. The same give and take is also seen in the religious sphere. The whole Vedic pantheon has been changed and augmented and it is impossible to say how many non-Vedic gods are worshipped in India today.

In this cultural process different ideas become the centre round which all cultural concepts are grouped. Since post-Vedic times Brahmanism and Kshatriyism have been two such concepts. Everybody gets possession of a Gotra and hastens to prove his Brahmanic origin, or all ruling houses, be they primitives or Rajputs or Dravidians, are direct descendants of the Sun or the Moon, claiming kinship with Shri Rama or the Pandavas. All agricultural and artisan castes call themselves either the one or the other kind of Kshatriya. These names do not indicate any racial mixture, but are merely a symbol of a cultural synthesis on an ideal plane. It is, therefore, interesting to find out what other ideas expressed the cultural synthesis of pre-Vedic times. As regards the occupants of India before the arrival of Vedic Aryans there is much speculation, for which there are no decisive data, but I think that the thesis put forward by Dr. von Fürer-Haimendorf helps to clarify many issues, and it is accepted for the purpose of this paper. He thinks that the Dravidians entered India at about the same time (2000-1000 B.C.) as the Aryans. The Dravidians entered *via* Baluchistan (the Brahui language in Baluchistan is supposed to be related to the Dravidian), wandered along the Indus southwards and embarked from some place at the mouth of the Indus and entered south India by the sea route. They consolidated their position in the south while the Aryans were colonizing the Gangetic plain. The inhabitants of India who lived here before the Aryans and Dravidians were pushed northwards by the Dravidians and southwards and eastwards by the advancing Aryans and so come to occupy the Central Indian forest and hill belt today. About A.D. 200 we find Dravidian literature full of Sanskrit words and concepts. Dravidian kings could



come as wooers for northern princesses (Kalidas, 'Indumati Swayamwar,' in *Raghuvamsha*, sixth canto) and were taking up the concepts of Brahmanism and Kshatriyism from the north. When we meet the Dravidians they are already Aryanized.

The pre-Vedic and pre-Dravidian cultural synthesis seems to have centred in the word 'Naga.' The earliest Sanskrit literature is full of the fights between Asura and Deva. Puranas tell of a fight between Deva and Danava as having taken place in olden times. These were probably fights between Aryan tribes in Iran and India. Apparently the Asuras were absorbed into the tribes of Devas and later Kshatriyas, and some seem to have taken shelter among the aborigines. In later literature the talk is about Nagas. The *Mahabharata* story is full of lengthy narrations about Nagas and their clans. The Kshatriyas were fighting with the Nagas or befriending them. The Vedas speak of aboriginal noseless people and call them Dasyus. The *Mahabharata* speaks of Nishad or Shabara as forest hunters, Das or Dasha or Kaivarta as fisherfolk of the forest and Nagas as a ruling people. Just as the Hindus are either Brahmins or Kshatriyas, so these tribal folk call themselves *Nagbansi*—descendants of Nagas. 'Naga' thus appears to be the idea which symbolized the cultural synthesis of pre-Vedic days. Of the many Naga kulas (clans) mentioned in *Mahabharata* a few can be regionally placed. Takshaka is the name which occurs again and again. The Kuru clan of Hastinapura records a marriage with a princess of the House of Takshaka. She was named Jwala. Another king married a princess called Nāgā. Arjuna burnt the Khandava forest to build the new capital of Indraprastha. Khandava forest was the home of Takshaka Naga, who vowed vengeance and killed Parikshit, the son of Arjuna. The son of Parikshit, King Janamejaya, started a massacre of Nagas which was stopped by Astika. So, woven into the main *Mahabharata* story, we have a story of a feud of three generations between Pandavas and the Takshaka Nagas. The story also makes clear the way in which these forest dwellers were ousted from their forests and driven ever eastwards and southwards. The Aryan Kshatriyas burnt the forests, built their cities and gave the land to their subject cultivators. Takshakas, being the westernmost Nagas, had to bear the whole brunt of the first Aryan attack and either perished or went eastwards. Arjuna married a Naga girl, Ulupi, living somewhere on the middle reaches of the Ganges.

In *Aranyak Parvan* we are told a story of much older times, the story of Nala. Nala was wandering in the forest somewhere north of Vidarbha (the present Chhota Nagpur area) and saved Karkotaka Naga from a forest fire. Karkotaka changed his shape and colour and bid him go northwards to King Rtuvarna of Ayodhya. Karkotaka is given in *Mahabharata* as one of the clans of Nagas. The country of Karkotakas is still to a large extent a forested land where Oraons, Bhuias and Mundas cultivate rice in small forest clearings. Among the Oraon clan names is one called 'Karakatta' meaning elephant. In *Mahabharata* the words serpent (*naga*) and elephant (*hastin*) are interchangeable. Thus Hastinapura, the capital of the Kuru clan, is also called Nagapura. Karakatta as a clan name for a Naga

seems very appropriate. This, however, may be only a coincidence. The Mundas also call themselves Nagbansis. Is this some new fashion or has it an ancient origin?

In *Dhammapadamhakattha* of the Buddhists is told the story of the destruction of the Lichhavi people at the hands of Vidudabha the son of Pasenadi of Kosala. Pasenadi wanted to marry a princess of the clan of Buddha so as to be related to him. He let his intentions be known to Mahanāma Sākya, the leader of the clan. A clan council was held and it was decided that a princess of Sākya blood could not be given to Pasenadi, but as a refusal would bring destruction, a girl of Mahanāma born of a maid (*Dasi-putri*) was given to him as a princess; her name was Nagamunda. This Nagamunda appears to be a woman from the clans of Nagas and Mundas. The Baigas are supposed to have descended from a Nanga Baiga, which is another term for Naga Baiga. *Nanga* is a term used for *naga* or serpent by the peasantry of the west coast of Maharashtra. The reference to Kajalibana Pahad especially lends support to this interpretation, as in Marathi folk songs Kajalibana (a forest as dark as the lamp-black ointment put in the eye) is always connected with Naga or Nagoba. The Kolis of Maharashtra call themselves Nagbansi. The Gonds call themselves Koi or Kui, an obvious derivative of Koli. We have Kols in Chhota Nagpur hills. The Kols or Kolis are mentioned as Kolla in Sanskrit literature and are called a warrior tribe. Who the Bheel or Bhilla were cannot be determined. They may represent the north-western Nagas driven southwards into the Aravali hills. A seventh-century Pallava inscription (*Ancient India, Bulletin of the Archaeological Survey of India*, January, 1949) mentions Munda rastra (the country of Mundas) as somewhere near Nellore in the Madras Presidency. The Mundas, the Kollu, the Bhilla, the Gonds apparently all tried to show their affinity to the Nagas. Today the name Naga is preserved only among the Nagas of the Assam hills. The Mundas, Bhilla, Gonds, etc., do not belong to the same race. Some are definitely Mongoloid, some Veddid and some Australoid. The name Naga is not the sign of racial mixture or racial homogeneity but a symbol of a cultural synthesis. The pre-Vedic age may thus well be called the Naga age of Indian history. Who the real Nagas were is a problem awaiting investigation.

It may well be asked how a once ruling people could be reduced to the present status of the primitive forest dwellers. This leads us to another feature of the Indian cultural process, that of 'progressive primitivization.' There is no caste or tribe in India which has escaped this process. When the Vedic Aryans and Nagas met, there was not much difference between the cultural levels of the two: the Aryans were pastoral nomads, the Nagas cultivators of rice in jungle clearings, and both were without a written language. But by the time the Aryans had acquired the alphabet and learnt rice-cultivation the Nagas were driven into the forest land to live as a primitive folk. In Indian history it is not surprising to find a ruling people reduced to poverty. The Gujars belong to the clan of the Gurjara Pratihara who founded a large empire in western India. They gave their name to whole regions like Gujranwala



and Gujarat in the Punjab and the province of Gujarat; but by the time the Pathans came into India the Gujars were reduced to the status of a nomadic people moving from place to place and living in temporary shelter. The same Gujars are today classed as not very skilled agriculturists in the Delhi region. In pre-Muslim days this primitivization was experienced by some clans which were thrown out of their position of power by other clans.

Kingdoms and ruling clans declined and new ones took their place, but whole regions were not thus thrown out of gear until the Mohammedans came. Asoka conquered almost the whole of India but left the kings of each region as rulers of their region paying tribute to the central power; he had no quarrel with the other regions, their gods or their peoples. This old mode of conquest vanished with this new people, who put everybody to the sword; no quarter was given to those who surrendered. Indian temples were ruthlessly destroyed and some regions which were once proud in their achievement became so lowly that even a discerning man like Hunter judging them from their present state of backwardness wrote a harsh judgment of the people of Orissa. Before the coming of the Muslims it was the monarchs who fell, but now gods and men fell together and a general primitivization set in in India. Those who turned the Muslim tide, like the Marathas and Sikhs, did so at a heavy cost. The Marathas ruled, but could not erect a single monument to the glory of their gods as their ancestors had done; so much time was not given them in their long and arduous struggle. This process of primitivization can be seen everywhere. In some regions like Bihar and Orissa it appears as if the Hindu cultivator lives in a fossilized state. In others the people seem to have reached a real pristine primitivity, as among some jungle folk, but a study reveals that their present mode of life is not a continuation of the past but a state of decadence. In certain regions, as among Sikhs and Marathas, a certain cold-bloodedness, a disregard for human life, is the sign of this process, while in others it is evident in a senseless accumulation of money without the customary expenditure on cultural grandeur or rather without even a suspicion of cultural values, as in the case of some Gujratis and Marwaris.

A social process (that of juxtaposed communities) of the type described above makes for infinite variety and infinite

tolerance. Live and let live is its guiding, though unconscious, principle in life. This is not to say that the other way of life is understood or finds sympathy; it merely means that the other is allowed to live as a matter of course. This has given rise to some salient features of Hindu culture. The idea of Brahman (the pantheistic principle of creation) and the worship of many deities can live side by side only in such a society. The traditional nature of moral values and the relativity of moral concepts is such an everyday lesson that the dichotomy of good and evil found in some religious systems completely disappears. The standard of life is set by the pattern of one's own social cell—the caste or the group—so that theoretical liberalism co-exists with a very narrow and strictly prescribed code of social behaviour. Everybody has a right to live his own way provided that way is traditional. Whether one has one wife or many, or one husband or many, whether one eats meat or shuns it, whether one worships Shiva or Vishnu or Sing Bongo, whether one wears a bodice or goes without one, whether one covers the head or not, are matters for each community to decide. In such a society cultural unity is achieved through exchange of ideas, so that in all varieties certain unities ultimately emerge. There is hardly any cultural choice where one trait is repressed and another substituted in its place. At all times, all or almost all the varieties of beliefs and institutions are in existence. The cultural unities are achieved because people one after another adopted certain ideas which became common to all; there is no central authority, religious or temporal, to forbid one type of things and prescribe another. The weakness and strength of such a society lie in this cultural process. The weakness is obvious. It cannot resist as one, but then it also does not fall as one. Its semi-independent cells may succumb one by one, but the adjacent cells go on living. In this lies the hope of another revival, of another cultural rebirth. Whether it is going to be in terms of the old process of multi-cultural unity, or whether, as is the fashion nowadays, it is going to be patterned after one idea, only time will show. The forces of modern times seem to be all on the side of one pattern. We are living in an age of monotheism, dictatorships and machines. The first patterns the religious and moral life, the second the social and political life and the third the material life.

## OBITUARY

**John Fee Embree, 26 August, 1908—22 December, 1950**

**233** The death of John Embree in a car accident on 22 December, 1950, robs me of a close personal friend and removes from anthropology one from whom I, at any rate, expected a major contribution to our studies.

When John Embree entered the Department of Anthropology of Chicago University he was already acquainted with Japan, of which his father, Edwin Embree, at one time in the Rockefeller Foundation and a supporter of anthropological studies in many different countries, was an enthusiastic admirer. The Chicago Department of Anthropology accepted a suggestion of mine that we should do our best to organize systematic studies of communi-

ties in Eastern Asia, including China, Japan, Indo-China, Korea and possibly Manchuria. The first appointment under that scheme was that of John Embree as research assistant to make a study of a selected Japanese community. On my way to China I spent a few very pleasant days with Embree in which we decided on Suye Mura in Kyushu as fulfilling the conditions for a suitable first investigation. John and Ella Embree took up their residence in that village, and on my way back from China I was able to see Embree and discuss his work. There are difficulties in such an investigation of a kind that are not met with in ethnographical studies in other parts of the world, but these were successfully overcome by John and Ella Embree.



The results of the investigation were incorporated in a doctor's thesis, and this was revised and published in America under the title of *Suye Mura*, and in England as *A Japanese Village*.

The war between Japan and China and the later more extended war, together with my removal from Chicago to Oxford, brought to an end our scheme for studies in Eastern Asia, though some of the Chinese students were able to continue their work from a centre in Kunming. When Japan attacked the United States and so brought them into the war, anthropologists in that country were called up for national service, and John Embree was allotted to make a study of Japanese Relocation, the process by which persons of Japanese origin were removed from western coastal districts into concentrations further inland. Embree was thus

diverted, as were many other American anthropologists, into the field of applied anthropology. It is not yet possible to assess the total effects of this development on the study of social anthropology, so as to judge whether it has or has not been of advantage to our science.

After the turmoil and disturbance of the war Embree was able to return to the study of Eastern Asia, and concerned himself with the region of Indo-China, and more particularly Siam. His early death means that we shall never learn the results of his own studies of that region. For his record as an anthropologist we are left with his account of a Japanese village and not very much else that can be permanent. A very promising career has been brought to sudden end.

A. R. RADCLIFFE-BROWN

## ROYAL ANTHROPOLOGICAL INSTITUTE

### PROCEEDINGS

**Ancient Mining and Metallurgy Committee: Report on Examination of Miscellaneous Copper and Bronze Artifacts.** By E. Voce, M.Sc., Ph.D., F.I.M., Copper Development Association, with a note by Professor V. Gordon Childe, F.B.A. With four text figures

Small samples taken from the specimens were analysed spectrographically and examined micrographically, the spectrographic analyses being carried out and photomicrographs prepared by the courtesy of the British Non-Ferrous Metals Research Association. In assessing the analytical figures presented in the accompanying table, it must be remembered that the spectrograph is incapable of precise quantitative determination of concentrations exceeding a few tenths of one per cent. For instance, while it is easy to distinguish between a copper and a tin bronze, it is generally impossible to assess the tin content of the latter by spectrographic methods alone. Metallographic examination does, however, often throw further light on the matter.

(a) FRAGMENT OF AXE FROM THE SOCIETY OF ANTIQUARIES OF LONDON  
(Micro Specimen No. 43, Analysis No. 1)

This specimen was examined because the form of the fracture

suggested that it might have been cast in a chill mould. It was found to be a cast tin bronze showing a relatively small amount of  $\alpha + \delta$  eutectoid, and, therefore, probably containing about 8 per cent. or less of tin. There were numerous small uniformly distributed spheroidal inclusions of a dark colour as indicated in fig. 1. These were possibly lead, though some at least appeared to be non-metallic in character and were probably connected with the presence of antimony and arsenic, as revealed by the spectrographic analysis. Coring was not severe and the crystal grains were fairly small and equiaxial. Columnar crystals were absent. These observations suggest fairly rapid solidification without marked temperature gradients. It is probable that the metal was poured from a temperature not much above its melting point into a cold clay or stone mould. It does not seem to have been chill cast nor, on the other hand, to have solidified slowly under a fire.

Considerable corrosion had occurred on all surfaces, including that of the fracture. This had advanced along inter-dendritic boundaries, as indicated in fig. 2, and had, to some extent at least, spread along crystallographic planes. This was apparently in regions where local distortion of the metal had caused the occurrence of slip bands; such an area, albeit unaffected by corrosion, is visible in fig. 1.

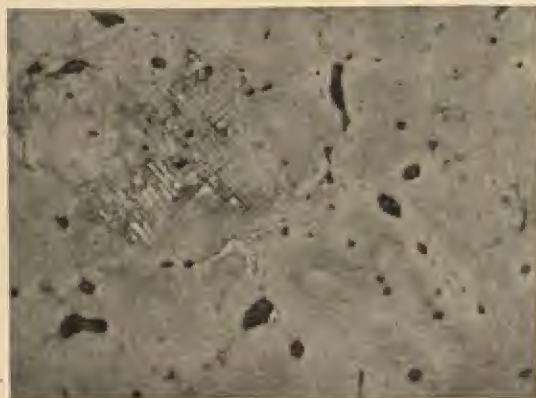


FIG. 1. AXE FRAGMENT FROM SOCIETY OF ANTIQUARIES OF LONDON ( $\times 225$ )

Lightly cored matrix with inclusions and small quantities of  $\alpha + \delta$  eutectoid. One crystal presents slip bands due to local distortion.

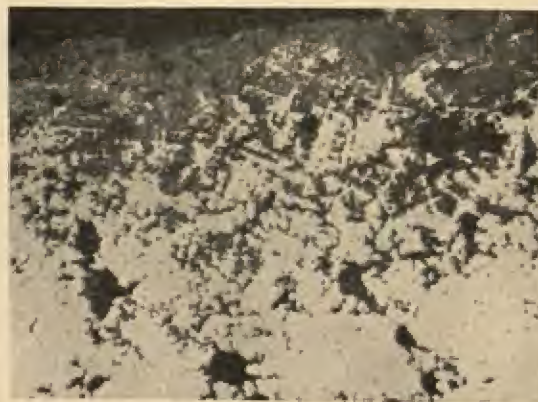


FIG. 2. AXE FRAGMENT FROM SOCIETY OF ANTIQUARIES OF LONDON ( $\times 37.5$ )

Photomicrograph of a section of surface showing penetrative corrosion



## Analysis

Tin	present
Antimony	~1 per cent.
Arsenic	~0.3 "
Bismuth	0.01 "
Cobalt	0.05 "
Iron	0.03 "
Lead	~1 "
Phosphorus	trace
Silicon	0.03 per cent.
Manganese	trace
Nickel	~0.2 per cent.
Silver	~0.05 "
Zinc	not detected
Tellurium	not detected
Aluminium	trace

~ = probably about

~ = probably about...or more

E. VOCE

(b) AXE-HAMMER FROM THE INSTITUTE OF ARCHAEOLOGY,  
UNIVERSITY OF LONDON  
(Micro Specimen No. 90, Analysis No. 4)

Introductory Note by Professor Childe

It is generally agreed that the stone battle axes found principally in Northern and Central Europe and in the northern part of the U.S.S.R. are immediately imitations of copper weapons. The principal argument for this belief, in addition to the very metallic splaying of the blade, is the presence on many such battle axes of a low ridge in relief. This has been interpreted as a careful and deliberate imitation in stone of the seam which would be produced in casting a copper axe in a valve mould. Now several copper implements are known from the Middle Danube basin which, though small, agree very well in profile and section with the early

Jutland battle axes, but I have examined several of these in Hungarian and other museums and have never been able to observe a trace of a seam on any of them. Of course, the seam is normally removed by a careful smith in finishing weapons, but its absence from all available specimens does suggest that these might not have been cast, but shaped by merely hammering and annealing. This treatment would yield the splayed blade, but, if the Hungarian examples had been thus made, the imitation seam on the Jutland and other stone battle axes would remain unexplained. I was fortunate enough to be able to acquire for the Institute of Archaeology two such copper axes (fig. 3), from a private collection sold at Sotheby's in 1950, and submitted one of them to the Ancient Mining and Metallurgy Committee of the Royal Anthropological Institute. The Committee was able to arrange a metallurgical examination of this specimen by Dr. Voce, and his report shows that the implement, though made of very pure copper—probably therefore native copper—had actually been cast. An implement of this rounded form could not, of course, be cast in an open-hearth mould, so one must assume that the seam had been very carefully removed by hammering. Both specimens do show some traces of hammer marks on the surfaces. It is satisfactory to have this confirmation of the theory that the stone battle axes are immediately imitations of metallic models, and that these models should be sought in the Hungarian metallurgical province.

V. G. CHILDE

## Report

The specimen was one of two from the Danube basin, probably from Hungary, and may have been used as a model for a series of stone weapons which it resembles. These stone weapons have a carefully made ridge which appears to be an imitation of the parting line of a metallic casting, but no parting line was discernible on the copper specimen examined. Nevertheless it was important to ascertain whether the object was in fact a casting or whether it was a wrought product. The spectrographic analysis revealed a high degree of purity, and it is therefore evident that the raw material was native copper.

## Analysis

Tin	not detected
Antimony	0.0005 per cent.
Arsenic	not detected
Bismuth	0.0003 per cent.
Cobalt	0.0001 "
Iron	0.0002 "
Lead	0.0004 "
Phosphorus	not detected
Silicon	—
Manganese	not detected
Nickel	0.002 per cent.
Silver	~0.0005 "
Zinc	not detected
Tellurium	not detected
Aluminium	—

~ = probably about

A section taken from a position near the cutting edge showed the structure of a typical tough-pitch copper, with the cuprous oxide distributed as a network of spheroidized eutectic. There can therefore be no doubt that the specimen was originally cast. The eutectic network, however, appeared to have been distorted to some extent and it can be inferred that a moderate degree of forging, either hot or cold, had been applied. Superimposed upon the cast structure were large twinned recrystallized grains, indicating that the metal had been annealed. The twin boundaries were straight, and the material gave no indication of having been



FIG. 3. TWO DANUBIAN AXE-HAMMERS IN THE INSTITUTE OF  
ARCHAEOLOGY, LONDON  
Length of lower piece: 8 inches



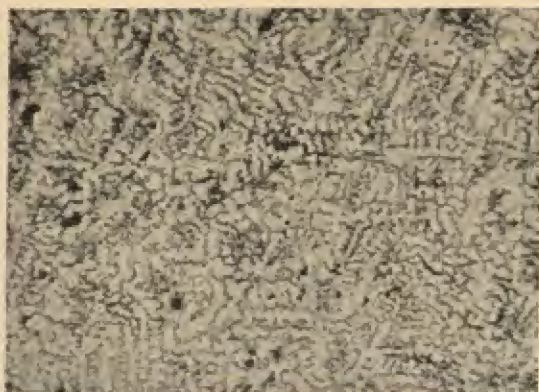


FIG. 4. 'SCEPTRE HEAD' FROM CASTLE MUSEUM, NORWICH ( $\times 225$ )  
Fine-grained cast structure, the two phases being presumably copper-rich  
and silver-rich respectively

distorted after the annealing treatment. The inference that the metal had been annealed is subject to the qualification that the observed recrystallization may possibly have occurred at ordinary temperature during the prolonged life of the specimen. Copper of high purity is known to recrystallize with comparative ease.

The section examined was remarkably free from porosity, slag inclusions or other defects.

E. VOCE

(c) 'SCEPTRE HEAD' FROM THE CASTLE MUSEUM, NORWICH  
(Micro Specimen No. 55)

Spectrographic analysis showed this object to be an alloy of gold, copper and silver, probably approximating in composition to 9-carat gold. A small quantity of tin, probably less than 1 per cent., was detected together with traces of iron, silicon, aluminium, antimony, arsenic, bismuth, cadmium and lead.

Metallographic examination of a small sample indicated that it was a casting. Two phases, presumably copper-rich and silver-rich, were arranged in a fine-grained dendritic pattern. The structure is illustrated in fig. 4.

E. VOCE

## REVIEWS

### GENERAL

**Caste: A Comparative Study.** By A. M. Hocart. London (Methuen), 1950. Pp. xvi, 157. Price 15s.

**235** This work of the late Professor Hocart was published in the form of a translation into French by E. J. Lévy and J. Auboyer in the *Annales du Musée Guimet* in Paris in 1938, and for an edition of the English original we have had to wait 12 years.

In a brief preface by Lord Raglan the basic theory of the book is lucidly summarized: all human communities were originally bodies of persons organized for ritual purposes, the chief of which was to secure a full life with sufficiency of whatever is necessary or desirable, including offspring; the divine king stands at the head of the rites and is god as well as man, and if his duty is correctly performed, not only will all men (and gods) receive their due but rain will fall in due season and the fruits of the earth be secured; the caste system is a system for distributing throughout the community the various duties connected with the royal ritual and the king's service, 'which are largely the same,' and ensuring that they are performed only by persons duly qualified 'both by heredity and by knowledge of the rites.'

To demonstrate this theory Hocart devotes a chapter each to India, Persia, Fiji, Tonga, Samoa, Rotuma, Rome, Greece and Egypt, and he adds a final chapter on 'Origins and Tendencies.' In this final chapter he sketches his theory that the societies of the countries named (and he would possibly have extended his theory, mentally at any rate, to all human societies) developed into their ultimate condition by a process of specialization in ritual performance and of concomitant secularization. Thus, he says, the priest has to shape the clay in the course of ritual to represent the things into which life is to be infused; he then limits his activities to the shaping of clay, and ultimately becomes a professional potter. In support of this he instances the claims of potter castes to be Brahman, and implies (p. 14) that the claim is generally accepted in India. It is true that potters are described by some ancient authorities as descended from the offspring of a Brahman father and an Ugra mother, though by others from Viswakarma, the divine architect, and a Sudra mother; that they officiate as *pujaris* in the temples of the village guardian, Ayanar, in Southern India; that some Tamil potters wear the sacred thread of the twice-born; and that some Mysore potters returned themselves at one census as Gundu-Brahman. They are not however generally accepted in India as anything but Sudras, and not always even as clean Sudras.

Hocart goes on to put the view that 'secularization is merely a form of specialization; it involves a narrowing-down of attention and interest.' He questions whether the process can ever work in the opposite direction, and a carpenter, for instance, give up pure

mechanics for a belief in the divinity of his tools; until actual cases are produced, he says, we cannot take such a theory seriously. Specialization, he suggests, goes hand in hand with what he calls, for want of a better term, 'nationalization' ('socialization' might have served better)—'the Indian barber and washerman are pretty well nationalized; everyone who is of good standing makes use of their services'—but that is not because they are necessary on technical grounds, as anyone can cut hair, and washing is easy; the reason why specialists are needed 'lies in ideas about birth, menstruation, and death,' and the spread of the use of specialists through all classes is due to snobbery and the fact that 'everyone likes to imitate his betters.' The royal ritual filters down to become the daily practice of the common people. Crafts are freed from their ritual setting and become the livelihood of specialists. Society becomes differentiated and specialized and needs a much greater degree of co-ordination.

The preceding nine chapters illustrate the working of this principle in the societies named. It is of course an easy matter to find parallels in different societies of the ritual aspect of individual or of communal relations. But Hocart argues his case with learning, logic and ingenuity. In some of the cases described the connexion may be due actually to diffusion; it is difficult not to see the same word in the Tamil *ambattan*, barber, and the Fijian *mbota*, the man who may touch the chief's head. But this ritual theory alone does not explain caste as seen in Indian society over the last thousand or two thousand years, even though it may add much to our understanding of it. As an explanation of the development of the Indian caste system, which (pace Hocart, who had little experience of it) is found nowhere else in the world, it is no more adequate by itself than Nesfield's theory, say, or Risley's. It is no doubt true, as Lord Raglan insists, that religious considerations count for more than economic ones with the greater part of mankind, but it is obvious to anyone that with very many individuals in any society economic considerations, considerations of personal advantage, outweigh any others, and to ignore their part in the development of social institutions is just as misleading as to rely on them alone, as economists perhaps have tended to do. It is not impossible that Hocart is right in his speculation 'that all jewellery began as ritual accessories'; the same might conceivably apply to clothing (a view which incidentally is supported by the third chapter of the Book of Genesis), but to extend it to the whole structure of society would reduce us ultimately to the absurdity of supposing that our human or subhuman ancestors of remote antiquity fed and fought and mated for ritual reasons alone.

It is perhaps a pity that Hocart's book was published under the title *Caste*. It is an interesting and suggestive aid to the study of



caste, as of stratified society in general, and a valuable contribution to inquiry into social origins, but it neither gives an account of nor accounts for caste as the system on which society in India has long been based.

J. H. HUTTON

**Social Anthropology: The Science of Human Society and Culture.** By J. S. Slotkin. New York (Macmillan), 1950. Pp. xviii, 604. Price £1 16s.

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This work uses the comparative method freely to compare categories of behaviour, types of relationships, and general concepts from all types of cultures at various times in all or any part of the world. The author writes his own descriptive generalities to cover the points compared. Any reader's impression will probably be confused at first inspection or reading of the book. The author should probably write another book in plain style to say what he has discovered by the use of the method. The present work is difficult to read on account of the masses of excerpts used.

In Evans-Pritchard's work on the same topic, with the same title (but not sub-title), some reference is made to Samoan studies illuminating American situations; Azande studies Russian; and Trobriand studies British. That kind of claim I do not consider to be valid, since direct study of any particular society is preferable for the purposes of knowledge. In Slotkin we find comparison of short excerpts from John Donne's sermons and Manus (Admiralty Islands) religion, but no study of how missionaries and primitive religionists act together when these are not being comparative but saying that comparison is invidious. Slotkin compares Azande ritual with the Christian rites of consecration and deconsecration of churches and not with anything specially Russian. The idea of comparison is engaging, but when categories, types and general concepts alone are compared one gains an impression that reality has been disembowelled. As far as science is concerned the study should have more body to it.

R. F. FORTUNE

**Dating the Past: An Introduction to Geochronology.** By Frederick E. Zeuner. 2nd edition, revised. London (Methuen), 1950. Pp. xvi, 474, 24 plates. Price 30s.

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A second edition of this indispensable reference book is very welcome, because the growth of knowledge in many branches of the subject has been rapid during the five years which have elapsed since its first publication. The text and many of the tables have been revised in the light of research published up to the end of 1948, and important data which became available during 1949 have been inserted in the Appendix. The classified bibliography has been expanded from 33 to 46 pages.

## PHYSICAL ANTHROPOLOGY

**Génétique et Anthropologie des Groupes sanguins.** By R. Kherumian. Paris (Vigot), 1951. Pp. 92, 36, 6 figs.

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Professor Kherumian, although well-known to physical anthropologists, is, comparatively speaking, a newcomer to the study of blood groups. Despite his disarmingly modest claims, he has made himself thoroughly familiar with this complex field of investigation. This is in itself no small feat, since much of the available information is scattered and undigested—as it is in any rapidly expanding branch of science which touches on so many aspects of research: blood-group serology, genetics and anthropology. Moreover, he brings to the subject a characteristically French lucidity of thought and exposition, so that the work must serve admirably its two stated purposes: to enable his students at the School of Anthropology to obtain some understanding of this important and difficult study, and to convey to 'the public' the present status of research in this field, so little and so imperfectly known.

There is, however, a great deal of material to cover in the compass of 128 pages, and it is perhaps inevitable that so brief an account should be a compilation rather than an original contribution. Indeed, in many places the statements of others are recorded without comment, even when they are apparently contradictory. The author stresses, along with Dahlberg and others, the advantage of substitu-

ing for the older 'metaphysical' concepts of racial characters features which are simply genetically determined and can be studied objectively in their several combinations. In this plea he will have the support of many anthropologists, but he indicates in only the most general terms how the substitution is to be brought about. In fact, he adds little to Boyd's illuminating review of the subject in *Genetics and the Races of Man*.

Convenient summaries of the effects on blood groups of isolation and intermingling of populations are given. It is a pity that the diagrams are so few in number and not always clear; in this sort of work, which is notoriously difficult for the inexperienced to read, good illustrations are most valuable.

This small book is accurate, well documented and modern, and it can safely be recommended as a full introduction to the subject. It is rather too extensive for use in our Schools of Anthropology, where comparatively little is taught about blood groups (although a more detailed study of genetical anthropology could, and should, be brought into the curricula). The research worker will still refer to the complete and scholarly work of Drs. Race and Sanger, *The Blood Groups in Man*, published last year, and to another book in the same series, on blood groups and anthropology, by Dr. Mourant, which we hope will soon be available.

A. C. ALLISON



**Du Crâne Animal au Crâne Humain.** By A. Delattre. Paris (Masson), 1951. Pp. 103

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In an endeavour to trace the mechanism of development of the skull to its human condition, the author examines a number of schematic cranial profiles, superposing them in a somewhat novel orientation. The plane chosen, that of the horizontal semicircular canals, is of functional significance both in man and in other animals, and is accordingly preferable to the frequently used non-functional conventions. By this method, it is

seen that cranial transformation to the human appearance is due to a rearward expansion of the occipital and nuchal areas, not to a general swelling of the skull such as is usually postulated. Other modifications of the skull and face are linked with these occipital changes, the whole being induced by the attainment of an erect position of neck and trunk.

Acceptance of the mechanism offered will depend on the results of its application to forms closer to the ancestral line of man than those used in the present study.

D. F. ROBERTS

## AMERICA

**Popol Vuh: The Sacred Book of the Ancient Maya Quiché.**

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Translated by Delia Goetz and Sylvanus G. Morley. London (William Hodge), 1951. Pp. 267, 4 illus. and map. Price 18s.

The *Popol Vuh* has long been known in the French translation of Brasseur de Bourbourg, and some later ones. The present one is made by Delia Goetz and S. G. Morley from the Spanish translation of Adrián Recinos. The Parra characters are not used in transcribing the Quiché words in it. It is regrettable that it is not stated how these letters are transcribed. The English rendering appears to be clear, but on p. 67 we read: 'with these invaders or rather before or after them.' This is meant to render what Landa wrote: 'they differ among themselves as to whether he arrived before or after the Itzas or with them' which makes sense, whereas the 'rather' makes the former passage meaningless. Also we are told that the Cakchiquel 'opened their arms to' Alvazado, which sounds a little odd.

However, these are small matters; the book is a good one and fills a long-felt want for English readers.

The history of the original manuscript so far as it is known is given and the various translations are mentioned. There is a useful account of the conjectural history of the Quiché, the Cakchiquel and the Maya, that of the first two being the best. It is in general satisfactory, though the author is somewhat inclined to make sweeping statements on matters which are by no means certain and to quote as authoritative some rather vague and platitudinous statements of earlier writers of the nineteenth and twentieth centuries, which really throw no light on the subject. I think too that such general remarks as that the *Popol Vuh* 'presents the complete picture of the popular traditions, religious beliefs, migrations and developments of the Indian tribes which populated the present Republic of Guatemala after the fall of the Maya Old Empire' are much too enthusiastic and require to be considerably toned down.

The suggested chronology for the Quiché also requires reduction. I think a better estimate of it was made by the late T. A. Joyce in 1914.

The original Quiché text was probably written in the sixteenth century and has probably been influenced in the course of recopying by scriptural and Spanish influences, yet it remains a very important relic, both from its large fund of mythological material and notably also from its eloquent literary style.

If one considers as a whole what Middle America has left to us one can see that the Maya monuments and codices have left no information except what the late Dr. Gann described to me in conversation as 'astronomical and calendrical piffle,' the Books of Chilam Balam a very thin current of history, the Nahuatl both in codices and in writings quite a substantial amount of history, some of it extending over very long periods, the Quiché much mythology and some history but no chronology in the Native Calendar, and the Cakchiquel less mythology and much more history, but the chronology is only from the date of the Revolt of the Tukuiche and is based on the Calendar, but in a unique form. Yet all these nations had the Calendar at least as far as the calendar round. And the Mixtec codices, on which Dr. Alfonso Caso is now working, show that they recorded names and dates over a very long history by the calendar round method.

RICHARD C. E. LONG

**The Book of the Jaguar Priest: A Translation of the Book of Chilam Balam of Tizimin.** By Maud Worcester Makemson. New York (Schuman, for Vassar College), 1950. Pp. 238. Price \$3.50

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Mrs. Makemson spent less than two years studying the Maya

language. For this she used only the Tozzer and Gates grammars and Motul and Perez, *Co-ordination alfabética*. She did not have the regular Perez dictionary, the Motul Spanish-Maya, the Vienna or the San Francisco. Her knowledge of Spanish is inadequate and she is incapable of transcribing the Maya text. There is no transcription in the present volume, but it appears by a translation made by her elsewhere of two pages of the Tizimin that she could not read the Maya text and that she mistook certain characters in it for others and did not recognize the cedilla under *c*.

Naturally with this imperfect background she has produced a so-called translation differing so widely from that of Royce, who has spent about forty years in Maya studies, and of Barrera Vasquez, who is a native of Yucatan (both of them substantially agreeing with each other in their translations), that it might have come from an entirely different book. As she gives no transcription of the text it is impossible to check her version. So far as can be gathered from the slender amount of material available, the errors in her translation appear to be very considerable.

The subject is sufficiently difficult in itself to require treatment by a writer with a much more profound knowledge of it than Mrs. Makemson has displayed. It is quite impossible to take this book seriously as an authoritative translation or indeed as being of any weight whatever in Middle American studies.

RICHARD C. E. LONG

**Culture in Crisis: A Study of the Hopi Indians.** By Laura Thompson. New York (Harper), 1950. Pp. xxiv, 214. Price \$4

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*Culture in Crisis* is the latest publication in the Indian Personality and Administration Research series (others include *The Hopi Way*, and *The Navaho*) and is the first monograph to deal with the second phase of these investigations, an interpretation of the findings at the administrative level. This volume concentrates on the 'crisis' in Hopi culture (though focusing primarily on First Mesa, Hotevilla and Oraibi) and attempts to answer the practical problem of planning long-range government policy and programme in terms of this crisis. Briefly, the crisis concerns the existing state of poverty, political disunity and moral degeneration of the Hopi. The scientific focus of the book is 'conceived as a structural analysis of the Hopi crisis in the context of the total pattern of human events in time and space relevant to it' (p. 15).

The first chapter is devoted to a construction, *in vacuo*, of a theory for the scientific problem and to an attempt to show its relationship to administration. The author's reliance on her own jargon prevents the reader from understanding clearly the meanings she is trying to convey. Miss Thompson's obscurity forces me to quote her directly. The theory is largely based on six definitions (called 'key operational concepts') and seven 'postulates' (pp. 15-18), typical samples of which follow.

## Key operational concepts:

'The structure of a culture is conceived as the basic, multi-dimensional emergent pattern of human events which comprise the culture in environmental setting.'

'A culture crisis is conceived as the manifestation of critical imbalance in one or more essential dimensions of a culture structure in environmental setting.'

## Postulate:

'The external world is an ordered world, bound by inherent law conceived as formative process immanent in nature, and



by dynamic causality in the sense that all nature is interconnected and every event an outgrowth of past events and a forerunner of future events.'

In the author's view the scientist's perspective must be 'as broad and multi-dimensional as the administrator's frame of reference,' implying that the scientist should assume the perspective of the administrator. But later on the same page (11), she imposes a perspective on the practical man, *viz.* that 'inner life needs, trends, and values should be the focus of the administrator whose aim is the improvement of welfare.'

To pursue research oriented in this manner, states the author, 'requires a marked capacity for . . . structural insight' and she further says that the 'solution of problems of dynamic structure depends on insight into exact patterning.' This constitutes the basic methodology employed in the book.

The next seven chapters deal with various 'dimensions' of the Hopi crisis, e.g. 'ecologic,' 'somatic,' 'sociologic,' 'psychologic' and 'symbolic.' Throughout these empirical chapters the author largely abandons her theoretical constructions and presents a good synopsis of Hopi history and social organization. She is, however, plagued with one dilemma which remains unresolved to the end. This is how to reconcile Hopi factionalism with her unyielding impulse to create a 'structurally balanced culture.' In doing so she fights a losing battle on her own ground. The evidence she presents from Titiev's *Old Oraibi* not only does not support her thesis, but contradicts it (see pp. 71 and 77).

Two solutions are offered to the dilemma, both unsatisfactory. One involves a tricky definition (introduced on p. 69) which shoulders a major theoretical burden. The term in question is 'correlative,' which in context is meant to imply a balance 'for the good of all' and is defined and distinguished rather incomprehensibly as follows:

'The term correlative is used in this inquiry to denote relationships of mutual dependency expressed by an interchange of equivalent but not identical values. It should be distinguished clearly from the "reciprocity" concept which, for purposes of this inquiry, denotes relationships of mutual dependency expressed by an interchange of identical values. The correlative relationship is basic to the Hopi social system and the correlative concept is basic to the Hopi world view.'

The other solution is to castigate the Oraibi group for having a social system which is 'out of kilter' and to brand the Hotevilla as 'archconservatives.' This leaves the author with the more congenial and traditional First Mesa group which, she argues, should be the prototype into which all other Hopi groups should be moulded. The latter is to be the job for the administrator.

ARTHUR J. VIDICH

### **Tribes under Trust: A Study of the Blackfoot Reserve of**

**Alberta.** By Lucien M. Hanks, Jr., and Jane Richardson Hanks. Toronto (Univ. of Toronto Press) (London: Cumberlege), 1950. Pp. xvi, 206, 7 plates. Price £1 10s.

It is refreshing to see the increasing number of monographs which deal with American Indians as they are today rather than as they were supposed to have been in the nineteenth century. The subjugation of the Northern Blackfoot followed the pattern common among the other buffalo-hunting tribes of the High Plains. While the buffalo were plentiful the Blackfoot were one of the most warlike of the Plains tribes but when their food supply was killed off they were induced to sign a treaty in which they ceded most of their hunting lands to whites and promised to be peaceful. In return they were granted a reservation of some 400 square miles on the Bow River in Southern Alberta, and the Dominion Government promised to feed them until they had become self-supporting farmers. Their progress toward civilization was to be guided by missionaries and an agent of the Dominion Government.

The first agents tried to make the Indians grow vegetables and corn on small plots of land. On the High Plains this was a hopeless plan even for whites, because successful farming requires a large capital outlay, a minimum farming unit of 320 acres, and concentration on dry wheat as a commercial crop. Cattle-raising was not much more successful, according to the Hanks, because the Indians

were afraid that their rations would be cut off if they accepted cattle from the Agency. In 1910 they agreed to sell half of their land. The 800-odd Northern Blackfoot of today are still supported by the trust fund established by the Agency through adroit sale of this land. The fund is doled out to the Indians in the form of rations, farm equipment, houses, medical services, pensions and other benefits. It gives them basic economic security and they are judged to be the wealthiest Indians of Canada, although this does not mean that they are wealthy by white standards. But they have not yet become self-sufficient.

In the old economic system wealth in horses was quickly accumulated and distributed to relatives and friends without much planning or thought about the future. The new economy requires foresight and a considerable capital outlay. The Agency has taken over all the responsibility for economic and political co-ordination for the community as a whole; the reserve is run by the Agency as a business enterprise for the welfare of the Indians. The Indians do the manual work in farming, cattle-raising and coal-mining, but they take little part in planning these enterprises. The old organization of bands has broken down, the council of chiefs is politically impotent, and the Indians are divided into factions. No one is grateful to the Government in spite of its sincere efforts to promote Indian welfare. Such discontent might be attributed to economic insecurity on some of the less fortunate reservations, but this explanation will not work for the Blackfoot. Paternalism is not enough, even when it brings economic security.

Indians on reservations want to be 'real Indians' and yet to have the material advantages of being white at the same time, an ambivalence which is unwittingly fostered by the Agency. At the mission boarding schools an Indian learns that hard work, foresight and Christianity will bring independence and prestige, but as an adult he finds that responsibility is not in his hands, and that his technical and moral training at school is not much help for the job at hand. Crop failures, lack of cash, cattle diseases, political impotence, sick children, the contempt of whites, pressure from relatives—all these things force him to accept the 'Indian Way' and complete dependence on the Agency. He soon feels that the Government owes the Indians a living, and accuses the Agency of graft, inefficiency and race prejudice. The Agent in turn says that the Indians are irresponsible and shiftless, 'just like children.' Yet in spite of their dissatisfaction, few Indians leave the reservation, because outside they would have to work for low wages for hostile whites, away from the companionship of relatives and friends.

If one compares the Indian problem with other situations of culture contact one might think that assimilation should have been relatively easy. The Indians form only a very small minority in a large white population; it is not a colonial situation. Indians are not in severe economic competition with whites. They are cared for rather than exploited. They are not treated as equals by whites, but they are not subjected to the violent race prejudice which Negroes suffer. But in spite of all this, most Indians are a miserable and discontented lot. The Hanks cite the Fox of Iowa as an exception, but recent fieldwork by students of the University of Chicago suggests that *anomie* is as prevalent there as elsewhere.

It would seem that it is very difficult to have islands of planning in a sea of free enterprise, especially when the people being planned are politically unsophisticated and the planners are government bureaucrats, outsiders. I suspect however that it is only this buttressing support from outside which makes the community planning possible at all. The Hanks suggest a cure: better training and more responsibility for the Indians, with a final goal of maintaining the Blackfoot as a community but organized by the Blackfoot themselves. One cannot make predictions in a situation of such complexity, but I suspect that no amount of technical training will give the Blackfoot enough political acumen or missionary zeal to maintain community planning in the face of the economic and social forces of the outside white world. They want too much to be like whites to run a social enterprise so foreign to white ways of doing things. Even their pride in being Indian is essentially a reaction to failure. The other solutions would be to go on as at present, which pleases no one, or to throw the Indians completely on their own, which the Hanks feel is impossible at this late stage. E. J. BOTT



**Social Organization of the Western Pueblos.** By Fred Eggan.  
Chicago (Univ. of Chicago Press) (U.K. Agents: C.U.P.),  
1950. Pp. xvii, 373. Price £2 5s.

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Dr. Eggan's book represents, in part, the fruit of Radcliffe-Brown's influence on American anthropology. With the exception of Spoehr's studies on the Indians of the Southeast, this is the first sophisticated study of kinship presented by one of Radcliffe-Brown's 'Chicago' students. Though the influence of his teacher is easily perceived in Eggan's work, this does not imply that Eggan has failed to contribute his own perspective and imagination to the interpretation of the Pueblo data with which he is concerned. It is out of these considerations that an extensive review of this book is warranted.

The primary subject matter of Eggan's book is the kinship system—or, as he prefers to call it, the social organization—of the Western Pueblos: Hopi, Hano, Zuni, Acoma and Laguna. Altogether there are seven chapters. One of these is an introduction which contains a statement of the problem, a theory for the study of social organization and a detailed critical review of the existing professional literature on the Western Pueblos, including the works of Kroeber, Lowie, Parsons, etc. The next five chapters deal separately with the five groups in question. Each of these chapters contains sections on what are called the kinship system, the clan system and ceremonial organization. In addition to this the chapter on the Hopi (who are treated most extensively) contains a section on political organization. The final chapter is devoted to a brief comparative analysis of Western Pueblo social structures and to a comparison of these with the social structures of the Eastern Pueblos and of other societies which, it is suggested, are of the 'Pueblo Type.'

The study is mainly concerned with two related problems: the nature and functions of social organization in each of the Western Pueblos; and the possibility of classifying these as varieties of a single specialized type of social structure and of explaining the variations in terms of sociological and historical factors. The author suggests that the study 'as a whole should increase our knowledge of the integrative and disintegrative factors in Pueblo social life . . . and that it might be used as a basis for other types of studies on the Pueblo. Lastly, the broader scientific problem to which the book is addressed is a comparison of types of social structure: Pueblo types compared to each other and to types found in other areas. Dr. Eggan, who since 1932 has made at least eight trips to the Southwest and has primarily focused his professional interest in this area, is eminently qualified to deal with these problems. His grasp of the empirical data is unrivalled.

It is a pity that only a few pages are devoted to his theory of social organization and his major concepts. The logical premises underlying his key concepts are left unexplored and in most cases, in fact, they are introduced on an *ad hoc* basis. For example, 'A society is a group of individuals who have adjusted their interests sufficiently to co-operate in satisfying their various needs' (p. 4). Underlying this definition of society based on needs is the suggestion not only that human needs are known but also that criteria exist to determine when they are fulfilled. Furthermore, if only those relationships which are based on an adjustment of interests are to be considered the 'society,' then a large sector of human behaviour which is based on conflict—war, long-standing disputes over property, internal disputes which lead to the fractionalization of the group, etc.—is excluded from the purview of the study.

Other definitions similarly restrict the types of behaviour which are considered to be relevant to the study: culture, for example, is defined as ' . . . the conventional attitudes and behaviour patterns by which the mutual adaptation and co-operation is carried out' (p. 4, italics mine). However, since the empirical part of the study does not rigidly adhere to these definitions, these shortcomings are not subsequently significant.

More important for our consideration are the conditions which Eggan establishes as being necessary for the validation of his study. These are as follows: that social structures, in order to make their classifications meaningful, have a limited number of forms; that the kinship system is equivalent to the social structure; and that comparisons of social structures must be made between phenomena of the same class or type. Since the book is concerned almost ex-

clusively with kinship systems and is largely a comparative analysis, its value as a systematic study is based on the extent to which these are shown to be legitimate criteria.

Eggan's reasoning in support of his contention that social structures have a limited number of forms appears rather weak. Going back to the concept of a social relationship it is stated that there is a ' . . . probability that there are a limited number of possible social relations and that these can combine to form a social structure in a limited number of ways' (pp. 5f.). A social relation is defined by its form—e.g. super-ordination, subordination, co-operation, caste, parent-child, buyer-seller, etc. Thus he appears to be less interested in the content and meaning of a social relationship than he is in the label which can be attached to it. In doing this he has accepted certain traditional categories of social relationships and has then used these to show that social structures have a limited number of forms. My criticism is not directed against the attempt to lay a basis for a comparative study. On the contrary, the argument involves the procedure Eggan uses to limit the number of possible forms. It would have been more palatable if he had openly stated the criteria which he considered to be important for such a study and, following this, had attempted to show that these were the best criteria for his purposes. His statement would then have read, 'given these criteria, social structures have a limited number of forms.'

The author calls his study *Social Organization of the Western Pueblos*. Since his primary concern is with kinship systems, he is compelled to show that for his data this social structure is equivalent to the kinship system. However, proof of their identity is not adequately shown. He says: ' . . . the kinship system has proved, in many cases, to be the most useful index of social structure. In many societies the kinship system represents practically the entire social structure, and, even where there are . . . segmentary and associational structures, these are frequently related to the kinship system' (p. 10). No arguments or empirical data from other sources are presented to show why this should be the case. A kinship system 'consists of socially recognized relations between individuals who are genealogically and affinally connected . . . ' (p. 10); one is left wondering what is the author's precise conception of 'social structure.' It appears that Eggan has imposed his notion of a kinship system on the concept of social structure and has thereby given the latter a restricted meaning.

It can hardly be disputed that comparative studies must be made on the basis of phenomena of the same class or type. The crucial problem here is to determine the criteria for establishing similar classes or types. As soon as the criteria are selected, the classification follows. Then the only grounds for dispute pertain to the criteria, not to the classification. Eggan tells us little of how he selected his criteria and, in fact, in his theory he mentions only a few of those which he later uses.

A systematic statement providing a rationale for the selection of his criteria would have contributed greatly to the comparative study of kinship systems and might also have shed some light on the utility of classifications in general. Brew, it will be recalled, did this very effectively in his study *The Archeology of Alkali Ridge*. Such a statement would also have offered a basis for justifying his criticism (pp. 291f.) of Spier's 1925 classification which was based primarily on the arrangement of cross-cousins.

A major conclusion of the study is that 'once the major principles of grouping are selected—by borrowing or otherwise—the elaboration of the kinship system takes place within rather narrow limits' (p. 297, italics mine). Among the Western Pueblos 'this structural development seems to have proceeded along parallel lines . . . , influenced at times by intermarriage, migration, trade and other contacts and affected by the particular base with which each began' (p. 297). As a general principle, this is fine, but it does not follow from the empirical material which Eggan himself presents. In his discussion of modern changes among the Hopi (pp. 133-8) of New Oraibi, he offers the following statements which tend to contradict his general principle: 'The tendency is toward separate households for each elementary family'; 'Ownership of houses has been shifting from women to men'; 'The clan as a social and ceremonial group has lost much of its significance'; 'The term "cousin," which is foreign to the old Hopi system, tends to be applied to all cousins';



'Changes in terminology represent not only a reduction in extension but also a shift toward bilateral treatment of relatives'; and 'Even within a lineage unit it is possible for several sub-systems to exist at the same time, with resulting inconsistencies in terminology and behaviour.' It is difficult to see how changes such as these can be construed as falling within 'narrow limits.' In fact, one can almost infer from some of these changes that a near minor revolution has occurred in the distribution of privileges in New Oraibi society. Furthermore we may legitimately ask if these changes parallel those among the more traditional Hopi groups or the now 'disintegrated' Laguna.

Despite these criticisms, this book is definitely well worth reading. It is a most sophisticated comparative study of formal kinship systems. As such, it offers a basis for evaluating the place of such studies in anthropology.

ARTHUR J. VIDICH

**Navajo Weaving, its Technic and History.** By Charles Avery Amsden, with foreword by Frederick Webb Hodge. 2nd ed. Albuquerque (Univ. of New Mexico Press), 1949. Pp. xx, 263, 123 plates. Price \$10

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Much work has been done on the textiles of the Southwestern States—for example, by Gladys Reichard, Ralph Mera, Mary Lois Kissell and the staff of the Laboratory of Anthropology at Santa Fé—since 1929, but without superseding Amsden's valuable book, of which a second and improved edition has now appeared. Among its many merits should be reckoned the author's interest in technical process (in which he follows W. Matthews and quotes from him freely), and his sense of the relation of design to technical necessities; and his equally strong emphasis on the economic aspect of Navajo weaving, past and present. Connected with this is the question of the upgrading of Navajo sheep stocks by the Bureau of Indian Affairs as well as by enlightened traders: should the purpose of this be to produce better wool for the Indians to sell as a raw material, or better and more suitable wool for their own industry? The Rambouillet merino which some officials have favoured yields wool too curly and too greasy for Indian tools and methods.

The set-up of the Navajo loom, the working of twills, diamond twills, and the surprising two-faced weave are lucidly explained, and there is a good chapter, historical and technical, on dyeing. Amsden concludes (p. 87) that:

'native dyeing was in an unstandardized, experimental state until the very moment of its extinction by commercial aniline dyes . . . that the Navajo were pioneers in the development of native colours for use on wool. We know that European sheep came into the Southwest less than four centuries ago; that the two-faced blanket weave is of very recent origin and the diamond twill is found only in textiles of the last half century [i.e. after 1879]; that Navajo design has undergone a steady and rapid evolution [striped—terraced—diamond—bordered]; and now we have this strong suggestion of constant experimentation with natural colorants: all pointing to the conclusion that Navajo weaving is a late and largely independent development, thwarted while yet in full career by the constantly increasing pressure of an alien culture.'

The revival of vegetable dyeing since about 1920 is shown to be due to the enterprise of certain traders who wished to cater for a more intelligent class of customer, though it has been welcomed by the best of the women weavers. (I remember persuading a Hopi embroiderer in 1912 to dye his woollen yarn with the traditional *bigelovia* flowers; he grumbled, but approved the result, remarking shrewdly that this was 'not very green, but the sort of colour Americans would like!')

Of the sociological setting of the art Amsden says little. Navajo weaving, he shows plainly enough, was learned from the Pueblo people; but how did an art so exclusively masculine in the pueblos become so entirely feminine among the Navajo? Perhaps because the latter acquired the weaving art with wool and the sheep after they had acquired the horse, and the men were absorbed in war and raiding? As in some Saharan communities, the feminine *biens de justice* had to balance the masculine *biens d'injustice*.

The 123 plates, some in colour, figure almost every old and valuable blanket existing in collections in the United States.

BARBARA AITKEN

## CORRESPONDENCE

**Art for One's Own Sake.** Cf. MAN, 1951, 165

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SIR,—Apropos the exhibition of Traditional Art from the Colonies, you say: 'unfortunately, the tribal artists are not available for questioning in connexion with this exhibition . . .' That is a defect that could well be remedied in a future show. I would reject, at least as far as South-East Asia is concerned, the second part of the same sentence: 'if they were available [they] would certainly not be articulate in terms of Western aesthetic theory.' I have spent much of the last five years sitting among people carving wood, working patterns in iron, making fascinating hats or necklaces of beads. I have sat in their houses, they in mine. Once having acquired a common language, I was able to listen to the most extensive explanations of what they were doing and why. I would go further, and say that a Bornean craftsman or artist is more pertinently aware of his motives than his equivalent in Europe.<sup>1</sup> This is largely because he produces in accordance with a tradition (though this is constantly changing) and at the same time primarily for himself or his immediate associates. The things that are made for sale to other persons or groups normally carry much less interest and prestige, with a few exceptions (nearly all funerary).

For this reason it proved impossible to get fine examples of these arts for the exhibition. The Sarawak Museum has some splendid things, largely obtained during punitive expeditions under the three white Rajah Brookes or by other Governmental pressures in pre-Whitehall days. In my own small collection, the best things were collected during the latter part of the late war, when I was the first white man to reappear and had an exceptional prestige—which caused native chiefs to give me items of merit.

It is here that Mr. Henry Moore goes astray in an otherwise sound appreciation. He rejects the idea that any of it is 'art for art's sake.' He says: 'I feel certain that this never happens in primitive art.' I would as certainly state that in Borneo the reverse is the case. It is normally with difficulty that a Bornean artist can be got to make something for somebody else or for a ritual purpose, and he works for his own satisfaction above all else. He carves hornbill ivory into an earring, deer horn into the handle of a sword, the door of his room into a climbing monkey, because he wants it that way and because doing it gives him rather the same sort of satisfaction that Englishwomen get from knitting. Religion and magic come into it for some of the formal figures (e.g. outside long-houses) which are the responsibility of groups. But, again with a few exceptions, these are generally of low standard compared with the individual workmanship in making pineapple cloth or a bone powder flask.

The running Borneo figure<sup>2</sup> in the exhibition which Mr. Moore particularly singles out is one made in this individual mood. The tradition of exactly such figures as these continues to the present day among the Berawans, who ornament their cars, walking sticks, plates, rooms with such things—but are exceedingly reluctant to part with them. They have got used to my nagging them for such objects, however. Recently I went up the Tinjar and at one house harangued them boringly on the value of letting their best work be seen elsewhere. In the morning I found hanging above my sleeping mat a beautiful soft-wood carving of a virile male blowpiping a saucy monkey. One of the villagers had done it overnight—to keep me quiet, he said.

The peoples that I know in Borneo have little idea of 'art' as



such.<sup>3</sup> But they do not like to have a tobacco box or sun hat which is not ornamented. The people who own or wear things without finished designs are often regarded as lazy or stupid. To have finely patterned or worked things is a matter of pride, in those areas where the old prides still exist. I would modify the phrase 'art for art's sake' in its application to Borneo, and say that there is most emphatically and dominantly an idea of 'art for one's own sake.' And it is art of a high order, living, daily, and common to all classes.

I should add that all these arts are rapidly dying out.

Sarawak Museum, Kuching

TOM HARRISSON

Government Ethnologist and  
Curator, Sarawak Museum and Library

#### Notes

<sup>1</sup> The same appears to be true in certain parts of Nigeria, notably in the northern Ekiti region of Yorubaland. But probably neither group of artists would be guilty of the almost meaningless abstractions characteristic of so much of 'Western aesthetic theory.' Their art criticism is no doubt of a high order.—Ed.

<sup>2</sup> This piece is incorrectly said, in the illustrated handbook of the exhibition, to be from North Borneo. It is correctly shown under Sarawak in the catalogue, where, however, the Baram River (to which the collector, Charles Hose, attributed it) is misprinted as 'Banam.' It must surely, I think, have been made by Berawans on the Tinjar River, which is a branch of the Baram.

<sup>3</sup> It would seem that the difference between Mr. Harrisson and Mr. Moore (who was of course referring specifically to sculpture) may be more verbal than real.—Ed.

**False Bolas Stones.** Cf. MAN, 1950, 97

**247** SIR.—Professor van Riet Lowe publishes 'a discovery of great significance to workers in the field of prehistory in South Africa—and possibly further afield.' This discovery is of a hammerstone used by natives for roughening their grindstones. He also mentions Janmart's records of similar stones in Angola, and then says 'it is extremely interesting and important to note that bolas-type stones are being made and used in at least two areas of southern Africa today.'

Now, is this a discovery at all? Surely all grain-eating natives use or used similar stones. I have many times seen them used amongst the Nilotic peoples of East Africa. The name for this stone in Dho-Luo is *nyating*. It is used to roughen both the large grindstone, called *pong*, on which the grain is laid, and also the small grindstone called *nyapong*, used to crush and grind the grain. The use of bolas stones is unknown to African natives today. But some Africans have told me that most of the so-called bolas stones exhibited here are probably hammerstones, though many stones so used may originally have been bolas stones.

Stonham Museum and Research Centre,  
Kitale, Kenya Colony

H. F. STONEHAM  
Director

**The Framework of Prehistory.** Cf. MAN, 1951, 64, 119, 152 and 153

**248** SIR.—I fear that neither Dr. Daniel's article nor the subsequent correspondence has dealt with quite the point I raised in my review of Professor Piggott's book (MAN, 1950, 190). I was concerned with general terminology as applicable to chapter headings in that book, and to what might be called introductory and educational archaeology.

In addition to the need for new cultural and chronological frameworks in advanced archaeological studies, there can be no escaping the duty of archaeological scholars to initiate some few general terms that will be both cultural and chronological, and which will gradually replace Neolithic, Bronze Age, etc. It is not reasonable to suppose that the section of the public for whom introductory books are published, and for whom archaeological features are now often provided by the radio and the press, can be helped by 'Period IV' or 'Period D.' Nor can the non-specialist be always assisted solely by the indication of a date in centuries or millennia. Anyone who has to do with the promotion of interest in archaeology, and the safeguarding of discoveries, knows the usefulness, however misleading, of the old terminology that gives at once some idea of age and of cultural position.

The fact that cultural and technological stages are not contemporary throughout Europe should not obscure the value of the over-all culture/time trend. For Europe, it seems to me, we require a set of five or six general terms which would include what we now call:

Paleolithic (probably two terms required);

Mesolithic (the post-glacial hunting-fishing cultures);

Neolithic—Early Bronze Age (the food-producing cultures in which metal was unknown or a rarity);

Full Bronze Age (cultures with elaborated trade and economic systems); and

Late Bronze Age—Iron Age (the new, mainly migrational, cultures embodying changed economies, increased metal resources irrespective of type).

My notes in parenthesis are merely guides, not definitions. I do not feel able to say what the new terms should be, because the standard must first be agreed. Is it to be technological, as in stone-bronze-iron, or can it be in some other category still reasonably exclusive to the culture phase in question?

With regard to specialist and detailed terminology, I would strongly support Professor Piggott's recommendation for a direct application of the universal scale of millennia (MAN, 1951, 152). In the gradual rewriting of European prehistory that is continually in progress, clarity will be more easily achieved if token chronological systems have not constantly to be demolished or ignored. There can be no disgrace in the frank alteration of a date given in centuries, or quarter-millennia, but the shifting of a particular culture up and down a token scale would mean chaos.

The question of cultural definitions is one for which an agreed policy is certainly required. Are they to be regional names like 'Baden' or 'Wessex,' or site names such as 'Los Millares' or 'Skara Brae'? It seems, perhaps, that site names are preferable until the range and nature of a culture are fairly understood; then a regional name becomes more appropriate. An increase in the number of named prehistoric cultures is now greatly to be desired, especially in the British Isles and Western Europe. The characteristic material and the geographical distributions are in many cases largely known. Their definition and labelling would again aid clarification, dispense with variant concepts of 'Periods II or III,' and allow a direct correlation of named cultures, through common properties, from one country or archaeological province to another. In this last matter the archaeological terminology applied to the Ancient East sets an example.

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**Social Organization in Arnhem Land.** Cf. MAN, 1951, 76

**249** SIR.—In his recent review of *Art in Arnhem Land* (which has been published by the University of Chicago Press as well as by Cheshires, Melbourne) Professor Firth describes our discussion of the social structure in north-east Arnhem Land as 'rather tantalizing.' Its full analysis, which was out of place in a work on art, will be made available soon. This will be found rather disturbing: the complex and fluid nature of social organization in this region (see A. P. Elkin, 'The Complexity of Social Organization in Arnhem Land,' *Southwestern Journal of Anthropology*, Vol. VI, No. 1, 1950) will disturb the clear-cut and nicely docketed pictures of Australian social organization which have prevailed, at least outside Australia.

Meanwhile, in spite of the reviewer's doubts, the *mata*, or linguistic group, does exist 'almost as an entirely independent unit,' and membership of this, as of the *mala* or clan, is patrilineal. Moreover, one linguistic group may be common to several separately named clans, or, *vice versa*, one clan may include sub-groups inheriting different dialects. Further, the *mata*, the linguistic group, is the more important grouping, while the *mala* appears to be a collective grouping of less immediate significance to the individual. The *mala* is a classifying unit. Thus, of Dalwongu, a *mata*, it may be said, here is such and such a Dalwongu *mata*, and there another, localized in different areas. Each of these can be regarded as a separate *mata*, or collectively as an operational unit which in turn is a *mala*, a 'lot of people' or a 'group'—i.e. the clan, which has a special name. The north-eastern Arnhem-Landers nearly always identify themselves



and others in terms of *mata* in songs, stories and conversations. As a rule it is only when pressed that they allocate the specific clan name; even then they are not always sure of the correct name, and some of them are familiar only with the *mata* name. There is no doubt whatever of the *mata* to which each person belongs, but there is often a doubt concerning specific clan affiliations. In theory of course, the linguistic group (*mata*) should not be separated from the relevant clan (*mala*); but in practice this is exactly what does happen, so that the importance of the clan name recedes with the stress placed on the linguistic-group name. The *mata* is regarded as the more stable unit, an attitude reflected in the fact that clan names can be 'sold' or given away, whereas linguistic group names remain relatively constant.

In his otherwise outstanding discussion of social organizations (in *A Black Civilization*, 1937), Professor W. L. Warner confused some clans with linguistic groups (pp. 39-51), a mistake not easy to avoid, especially as he speaks of languages instead of linguistic groups, that is, social units; for *mata* (a term he does not use, but must have heard) literally means 'tongue' or 'language,' its broader significance being derived from this. Further, his phratry (pp. 33-35) or larger grouping is really the *mala* or clan, with its distributed sub-clans (which he regards as clans).

In his *Economic Structure and the Ceremonial Exchange Cycle in Arnhem Land* (1949), Dr. Donald Thomson not only omits many significant and relevant features of east Arnhem Land economy, and fails to describe the real ceremonial exchange cycles of the region (see for comparison R. M. Berndt, 'Ceremonial Exchange in Western Arnhem Land,' *Southwestern J. Anthropol.*, Vol. VII, No. 2, 1951); he also fails to define *mala* adequately, and omits altogether the *mata*, the most obvious and important social unit. He is confused, too, with regard to the nature and function of the clan and horde.

Professor Firth raises the problem of the linguistic relation of mother and child in this society with its patrilineal, patrilocal, exogamous clans and sub-clans, and exogamous patrilineal linguistic social groups. In spite of these aspects of the organization, a child learns his mother's dialect (*mata*), even though he is born and brought up in his father's 'country.' He is with her continually during his early years, and also with her co-wives, probably from the same dialect group, and his father's brothers' wives, who may be his mother's sisters, own or classificatory. So an east Arnhem-Lander speaks his mother's dialect, but he also learns his father's dialect—his own by inheritance, and during adolescence and early manhood uses it more and more; but he does not 'lose' his mother's tongue, for her brother and her mother's brother are of social significance to him. Actually, everyone can converse in several dialects learnt during childhood—often mixing them a bit. It should be noted that Arnhem Land clans frequently mingle in social and ceremonial life. There is not the long temporal and geographical separation of local groups which marks desert tribes.

This familiarity with the dialects of both parents, as well as with those of other relatives, appears to have brought about a situation in which today there is sometimes difficulty in allocating certain words to a specific *mata* (although each *mata* has its special characteristics of vocabulary and occasionally word order).

Professor Firth refers to the inconsistency in phonetics and use of terms used by various writers on Arnhem Land. Dr. Thomson's small book was, unknown to us, going through the press at the same time as *Art in Arnhem Land*; in any case we are not able to accept his renderings or uses of words as necessarily accurate, certainly not *malla* or *Wongo*. Two of us worked in the native tongue, and Dr. Capell of this Department has done linguistic research in the region. Professor Warner did not use sound phonetic transcriptions. Merit to previous and other workers, yes; but not perpetuation of inaccuracies! In *Art in Arnhem Land*, phonetic symbols had to be avoided. (Thus, Jirijja should have been Yirijja, hence the note that J=Y).

But sound foundations have been laid at Sydney for linguistic and anthropological research, and we can only hope that workers from other States and countries will collaborate.

A. P. ELKIN  
R. M. BERNDT  
C. H. BERNDT

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**Social Anthropology: Past and Present.** Cf. MAN, 1950, 198, 254 and 271, and 1951, 33-35, 62, 78, 120, 150 and 199. Sir.—Mr. Andrzejewski (MAN, 1951, 120) puts forward, as 'established sociological generalizations':

- (1) that the forms of power tend to coalesce, that is to say that those who have the political power tend to acquire the economic, and *vice versa*;
- (2) that extensive polygyny goes with great economic inequalities;
- (3) that war promotes autocracy; and
- (4) that over-population tends to produce war.

I would reply:

- (1) that the Medici and Cecil Rhodes are exceptions to the rule that the world's financial and commercial magnates have never exercised political power;
- (2) that there is no economic inequality in those Australian tribes where the old men have all the wives;
- (3) that in the 120 years before Waterloo this country fought a long succession of wars, but the government became more democratic; among savages the Masai were continually at war, but never produced a dictator; and
- (4) that, Bengal being one of the world's most overpopulated areas, the Bengalis should be one of the most, but are one of the least, warlike peoples in the world.

Mr. Andrzejewski's generalizations could no doubt be qualified in such a way as to become true, but only by becoming truisms.

Usk, Monmouthshire

RAGLAN

**Pictographic Writing in the Western Sudan.** Cf. MAN, 1950, 219

251. Sir.—The signs on the Ashanti gold ornaments, gold-weights, brass utensils and boxes, chiefs' appliqué cloths, the *Adinkera* mourning cloth, etc., are actually all pictographs, the meaning of which has, however, in many cases been forgotten. Of the few remembered I can refer here only to those which are identical with those shown in figs. 1 and 2 of M. Zahan's article.

Fig. 1. (3, Dogon meaning: 'drought') This is, in Ashanti, the sign for the *kra*, the life-giving power of the king, contained in his divine soul; (4, 'the sun') the face of the sun or moon; (5, 'the moon') the waxing moon; (6, 'a star') the points of the king, the four cardinal points; (7, 'the world or universe') resurrection and rebirth; (9, 'the jackal') the queen-mother's points, the equinoctial points.

Fig. 2. (4, Bambara meaning: 'death') Death; (5, 'the sun's movement') the zigzag denotes fire, and, since the sign is formed of two chevrons—two being the number of division and therefore of birth—this sign could be interpreted as the birth or beginning of the sun, i.e. the beginning of the sun's movement; (7, 'the four cardinal points') the symbol for the king as son of the sun, the four cardinal points; (10, 'fertility') the symbol of the queen-mother as daughter of the moon, i.e. as a Giver of Life (fertility); (11, 'creative motion') the symbol of birth and creation.

There are, further, the pictographs on the Fante fishing boats (fig. 1). The Fante, a branch of the Akan, learnt these signs from the

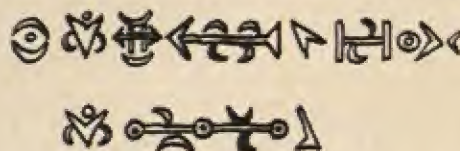


FIG. 1. FANTE FISHING-BOAT PICTOGRAPHS

Etsi, another branch of the Akan, whose home up to about A.D. 1000, according to their traditions, was along the Upper Niger River in the region between Djenné and Timbuktu. They were at that time a caste of fishermen serving the ancestors of the kings of Diala or Diula, later in the Gold Coast the kings of Bono, now Paramount Chiefs of Techiman. Their chief town in the north is said to have been on an island in the Niger River, which they call the Koraa.

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(a) CAT (LIST NO. 15) FOUND IN A HOUSE IN BLOOMSBURY IN 1915

*The pose is quite a natural one for the cat to have died in, and it may have been either accidentally walled up or deliberately placed in the building as a foundation sacrifice.*

*By courtesy of the Trustees of the British Museum (Natural History)*



(b) CAT AND RAT (LIST NO. 20) IN SALISBURY MUSEUM

*The group has been posed after death, and may have been intended as a rat-scare.*

*By courtesy of the Salisbury, South Wilts and Blackmore Museum*



# DRIED CATS\*

by

MARGARET M. HOWARD, F.Z.S.

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**252** In the course of the reconstruction or demolition of old houses a number of mummified bodies of cats have come to light. These have aroused the interest of archaeologists, and in consequence attempts have been made to obtain information as to their origin. Three possible theories have been advanced: (a) that the cats were placed in buildings as foundation sacrifices; (b) that they were intended as vermin-scares; (c) that they were accidentally enclosed, or trapped in cavities between walls, beneath floorboards, etc.

Since careful examination of recorded cases was likely to shed light on these theories, I undertook to collect relevant information. After some time I learnt that Mr. N. Teulon-Porter of Shaftesbury had embarked on a similar investigation. He very kindly suggested that I should complete the work and undertake its publication; I am very grateful to him for his generosity in handing over the material he had collected and for his permission to use it in this paper.<sup>1</sup>

The following list contains all the records of dried cats which Mr. Teulon-Porter and I have been able to collect.

## DESCRIPTIVE LIST

### *Datable specimens in chronological order*

(1) Cat and bird found at Hay Hall, Tyseley, Birmingham. Facing each other, the cat with gaping jaws and extended claws, in a square cavity about nine inches deep surrounded by heavy oak between the outside wall and the lath-and-plaster inside wall. There was no possible means by which the creatures could have gained entry, and the plaster of the wall at the back was undamaged. House probably built between 1275 and 1300, but inner wall perhaps a later addition. Specimens now in a show case at the works of the Reynolds Tube Co. who own Tyseley Hall.

(2) In the roof of a fourteenth- or fifteenth-century house, to the south of the parish church in Little St. Mary Street, Bridgwater. This cat was of ebony colour, its mouth open in a 'snarling' way, and its forepaws raised defensively as if striving to fight off an enemy. Specimen at one time in Bridgwater Museum but since destroyed.

(3) Cat with two rats, found beneath sixteenth-century woodwork in a house in Borough High Street, Southwark, London. It holds in its jaws a rat which appears to be struggling to escape, with its legs extended, its mouth wide open and its tail erect. Another rat, beneath the cat's forefeet, writhes upwards as if to bite its captor. Artificially arranged, since no accident could have killed all three simultaneously in such dramatic attitudes. Figured in *Illustrated London News*, 8 December, 1948; now in the possession of Le May, hop merchants in the Borough, Southwark, London.

(4) Cat found during demolition of the wall of a seventeenth-century inn at Usk, Monmouthshire. Lies on its side in what appears to be a fairly natural position. Specimen now in the Newport Museum and Art Gallery, Monmouthshire.

(5) Two cats (? skeletons) under the floor of a landing in an old house at Bampton, near Burford, Oxfordshire. House built in 1590, but staircase made in 1640, so probably landing also of the later date; originally floored with elm boards, but a later floor added on top. The cats were under the original floor; with them were three playing cards of Queen Anne's reign. These do not date the cats, however, since they may have slipped down through the cracks

between the floorboards before the second floor was laid down. Specimens probably not preserved; report based on a personal account from Miss Cecil Western.

(6) Cat found in 1950 during alterations by the Ministry of Works to a house built by Sir Christopher Wren probably between 1666 and 1723 in the Tower of London. Lying against a joist beside a corner fireplace under the floor of an upper room. Specimen in the Department of Environmental Archaeology, London University, Institute of Archaeology.

(7) Found during blitz repairs to the church of St. Michael Royal, College Hill, London. Built into a sealed passage under the roof, behind the moulding of the wall which runs round the church. As this passage had not been opened since 1691, the cat must have been placed there by Wren's masons when the church was built in 1687. The cat's head is raised and its mouth is wide open, a position in which it is unnatural for the animal to have died. Preserved in the church.

(8) Discovered in 1916 during alterations to 5, Dame Street, Dublin, when a lath-and-plaster partition wall between the hall and one of the rooms on the ground floor was removed. The house is one of a row of Georgian houses next to the Royal Exchange and about 200 yards from Christ Church Cathedral, where the cat and rat No. 9 were found. Mr. Mason, an optician, who owns the house, says the title deeds go back to 1786, when the house must have been new. In Mr. Mason's possession.

(9) Cat and rat, found when the organ of Christ Church Cathedral, Dublin, was moved from the roodscreen to the transept, during the restoration of 1872-78. D. A. Chart in *The Story of Dublin* says, 'These desiccated bodies were found behind the organ case in a dry and leathery but undecayed condition, and were posed in a glass case, so as to form a sort of tableau.' Specimens exhibited in the Cathedral.

(10) Bricked up in a partition wall of 5, Secretary's Lane, Gibraltar. Partition erected about 1879 by the occupant. About the same time, the pet cat of the family disappeared. Spanish masons appear to believe that the bricking-up of a cat in a new building brings luck. Specimen now in Gibraltar Museum.

(11) Cat found in 1946 at Dalbäcken Farm, Bjurtjärn parish, Värmland, Middle Sweden. Discovered when the steps to a front door, which had been put in place 25 years before, were removed. In a very contorted position, with head and tail raised, mouth wide open and claws extended, not at all a natural position of death. Specimen in the Uppsala Palaeontological Institute, Sweden.

### *Specimens which can only be dated as earlier than the date of discovery*

(12) In a house in Lothbury in the city of London, demolished on the construction of the Bank of England in 1803. The cat was between the wall and the wainscotting of a room, and had a rat in its mouth. Now in Sir John Soane's Museum, Lincoln's Inn Fields, London, together with No. 18.

(13) Found in Ashburnham House, Westminster School, London. Said to have been always referred to as 'Dr. Liddle's cat' and to have had a collar with his name on it. Dr. Liddle was Headmaster of Westminster School from 1846 to 1855. Specimen not traceable.

(14) Cat and rat found in the thatch of a cottage at Pilton, which was pulled down in 1890. The cat was pegged down with fair-sized wooden pegs, and as now mounted is about four inches away from the rat's tail, which it appears to be pursuing. Specimens in the Peterborough Museum, Priestgate, Peterborough.

(15) Cat found in a house on the Duke of Bedford's Bloomsbury Estate, London, during demolition of the property in 1915. It lies in a natural and undistorted position. Now in the British Museum (Natural History), South Kensington, London. See Plate Ka.

\*With Plate K



*Specimens which are not datable*

(16) Skeleton of a cat with some sinews still on it, found in the walls of a castle in Vadstena, on the shores of Lake Vättern, Sweden. Exhibited at the castle.

(17) Cat with mouse in its mouth, said to have been found in an old house in Tewkesbury, which is now Dobell's Brewery. Specimen untraceable.

(18) Cat found in Lord Yarborough's old house in Chelsea, and now in Sir John Soane's Museum.

(19) A 'plaque' of a cat said to have been found at Tamworth, Staffordshire. No details known.

(20) Cat posed bending over large rat. In Salisbury Museum. See Plate Kb.

(21) Large cat, in a stretched running position with mouth wide open, from an old collection of the Gibraltar City Council. In the Gibraltar Museum.

(22) Dried cat formerly kept at the Marquess of Granby public house, New Cross Gate, London, but now lost or destroyed.

(23) Cat formerly in a public house in the Phibsborough district of Dublin, also lost.

(24) 'Starved' cat from the stables of Beaufort House, recorded in Sir Hans Sloane's catalogue, No. 1887.

(25) A 'starved' cat found under a pew in St. Clement Dane's Church, on digging to make a grave, recorded as No. 1822 in Sir Hans Sloane's catalogue, and still in the collection in July, 1889.

## DISCUSSION

A careful consideration of the circumstances of the finds shows that each of the three theories can be substantiated by some of the material.

*(a) Foundation sacrifices*

The first group is that suggesting foundation sacrifices, with which may be included roof-tree sacrifices, since the motive in both cases is the same. The idea of foundation sacrifices goes back to very early days. They have been practised until recently in some primitive communities, where various animals and occasionally even human beings were used.

There can be little doubt that the sacrifice of a human being was the original practice. When St. Columba was building his church at Iona, he asked which of his disciples was willing to go down into the foundations to propitiate the spirits of the soil, who demolished nightly what was built during the day. St. Oran offered himself, and was interred beneath the church. Similarly, it is said that Clonmaclois was consecrated by the burial of a leper, who was in St. Patrick's retinue. In more civilized times an animal would be substituted for the human sacrifice; confirmation of this is found in local tradition, in folklore and in fairy tales, where the devil is said to demand his due of a soul on the completion of a building, and is fobbed off with an animal substitute. In England today, the custom is still observed of placing coins or other objects under a foundation stone, perhaps as a substitute for the animal sacrifice.

Foundation sacrifices seem to be meant as propitiatory offerings to the gods of the land, but sometimes a roof-tree offering was made to the forest gods, the sacrifice being put between the walls and the roof, or slaughtered on the roof-tree, and its blood allowed to run down the sides of the roof.<sup>2</sup>

A remarkably late example of this type of sacrifice is recorded by Mr. W. P. Rogers, of St. Agnes, Cornwall.

About 1890, an addition was being made to a cottage near Falmouth. One day, the work stopped; upon enquiry the builders revealed that a sacrifice would have to be made to the 'outside gods,' of a virgin hare trapped by a virgin boy. Seeing that the building would never get finished otherwise, the sacrifice was agreed to, provided that no cruelty was involved. Some years afterwards, during repairs to the roof, the remains of a rabbit were found in a beautifully made coffin near the top of the wall.

It may be noted that like the cat the hare is often used as a sacrifice at harvest festivals, and both are credited with being witches' familiars. It may be that a reputation for magical qualities accompanied the cat from the East, since the Romans, who appear to have introduced it into Britain, must have known of its sanctity in Egypt. In spite of the semi-deification which caused the Egyptians to have the bodies of their sacred cats mummified and carefully preserved in specially constructed tombs, cats also served in a quite utilitarian manner, as assistants to their masters in the chase of wildfowl.

In Wales, during the reign of Howel Dda, from A.D. 907 to 950, cats were appreciated as mousers, but this utilitarian treatment antedates the vogue of mediæval and post-mediæval superstition.

In the later Middle Ages cats fell on hard times. They were burnt on Shrove Tuesday in the Vosges and at Easter in Alsace. In the Ardennes, they were thrown into bonfires on the first Sunday in Lent, or roasted on the end of long poles, or in wicker baskets. Cats were drowned at Whitsuntide, and, in Oldenburg, Westphalia, Belgium, Switzerland and Bohemia, killed and buried during Lent. In Transylvania and Bohemia, a black cat was buried under a fruit tree to stimulate its growth. On Christmas Eve and at the sowing of the first seed, black male cats were killed and buried in the fields to prevent evil spirits from harming the crops. Cats were also sacrificed to end pests and other epidemics.

All these accounts show that, in the Middle Ages, the cat was regarded as a representative of the Devil, yet, at the same time, it had a magical value, since it was believed that its death could enrich the harvest and protect the crops and herds from evil magic, and bring prosperity to the individual. Attempts are recorded in Scotland to call up the 'cat god' by a cruel practice called *taghairm* in which black cats were impaled on spits and roasted.

Considering, therefore, the remarkable place which the cat has held in superstition, it is not surprising that the remains of cats should be found in positions suggesting that they were deposited as foundation or roof-tree sacrifices. Of the specimens known to me, Nos. 6, 7 and 10 may reasonably be so regarded; in other instances, this explanation is merely possible. It is noteworthy that the custom appears to have continued until quite recently: for instance, No. 10 was immured by Spanish masons in 1879, and No. 7 was intended by Wren's masons, in 1691, as a roof-tree offering<sup>3</sup>; No. 6, from the Tower of London, was probably immured by the same men for a similar reason. No doubt ideas on the subject had become somewhat confused by that time, and the men thought of their



action merely as being 'lucky,' rather than as a magical propitiation of the Devil. It is possible that these 'Wren cats' may have been due to a revival of the ancient custom, as a superstitious reaction to the Great Plague and the Fire of London. The workmen, during the rebuilding that went on after the Fire, may have felt a need to insure against the recurrence of the destruction.

It appears that, broadly speaking, the suspected cases of foundation sacrifices are later than those of the vermin-scares.

#### (b) *Vermin-scares*

The second theory, which Mr. Teulon-Porter considers the most probable, is that suggesting vermin-scares as a reason for the presence of mummified cats in buildings. Since effigies of men are frequently erected as scarecrows, it seems only reasonable to suppose that dead cats may have been put up as vermin-scares.

Of the examples listed, it can definitely be said that Nos. 1, 3, 9, 14, 17 and 20 were deliberately placed in position, since no imaginable accident could have preserved them in such lifelike attitudes. In No. 14 the cat had been fastened in place with wooden pegs, in a manner which proves the group to have been artificially arranged. In No. 3 the cat has one rat in its mouth and another one under its paws, and it is quite obvious that the group has been very ingeniously set up, as must also have been the groups Nos. 9, 17 and 20; while No. 1 consists of a bird and a cat dramatically arranged facing each other. There would be no need for such lifelike and elaborate arrangement in a foundation sacrifice, where the death of the animal sacrificed would be all that was required. It is difficult to imagine the reason for such elaborately arranged groups, which must have taken much time and trouble to prepare, but it may have been felt that a cat with one or more rats in its clutches was more awe-inspiring to vermin than a cat by itself.

Cat No. 3 is dated by the sixteenth-century panelling in which it was found enclosed and No. 1 may be of any date after 1275. The others cannot be dated with certainty, but No. 9 must be of about the same date as the organ of Christ Church Cathedral.

#### (c) *Accidental enclosure*

The remainder of the examples may belong to the accidental-enclosure category, and can, therefore, be given the benefit of the doubt.

#### CONCLUSION

In conclusion, it may be said that, in Egypt, the cat had both a magical and a utilitarian reputation. The Romans probably brought it to Britain because of its value as a catcher of vermin. That it was kept as a domestic pet is evident, since its remains have been found in more than one Roman villa and its footprints are preserved on numerous tiles, across which it must have walked when they were laid out to dry. In the tenth century, at the time of Howel Dda, the laws passed for its protection show that it was valued as a mouser and this appreciation may, later on, have led to the idea that the enclosing of a cat in the walls of a house would protect the building from vermin.

The setting-up of a cat with one or more rats in a realistic group would be a development of this idea.

During the superstitious, witch-hunting times of the Middle Ages, the cat acquired a reputation as a magical animal, the familiar of witches and the associate of the Devil. Cats had probably become very plentiful and were therefore easily available for use as sacrifices and offerings to their supposed master, the Devil, as the old gods had come to be called. The ancient idea of foundation sacrifice, to appease these powers of darkness, could thus, in due course, have become blended with the utilitarian conception of the cat as a vermin-scare and would result in a vague idea that the immuring of a cat in a new building was 'lucky.'

The evidence at present available thus suggests that, generally speaking, the cat was first immured for utilitarian reasons, but, having become an object of superstition, it came to be used as a luck-bringer or building sacrifice and also as a protector against magic or pestilence.

As to the method used in the preparation of these mummies, it is difficult to form an opinion. They may have been smoked or partially desiccated. The artificial arrangements and distorted poses of the animals imply some sort of preparation and mounting to keep them in position, such as may be seen in the pegged-down specimen from Pilton, No. 14. The fur has, naturally, disappeared as a result of partial putrefaction, which has caused the hairs to drop out of the follicles, but some of the long whiskers and eyebrows have usually remained, as also some long hairs on the forelegs which are hidden in life by the fur. The arrangement of the groups shows considerable ingenuity and a great deal of trouble must have been taken in their preparation. It may be worth noting that, while all the cats appear emaciated, the rats are large and rather bloated-looking.

#### Notes

<sup>1</sup> My very sincere thanks are also due to all those who have so kindly helped me in my enquiries. I must also thank Mr. Coysh, of the British Broadcasting Corporation, for letting me read the correspondence in connexion with the discussion on dried cats broadcast in 'Country Questions,' and Mrs. E. Ettlinger, for information on foundation sacrifices. Others to whom my thanks are due include Mr. H. Bowen, Dr. Fraser, of the British Museum (Natural History), South Kensington, Mr. J. P. L. Gwyn, Mr. McEwan, the custodian of Gibraltar Museum, Mr. Woodis Rogers, Miss Margaret Taylor, Mr. C. Wängsjö, of the Uppsala Palaeontological Institute, and Miss Cecil Western. Finally, I am indebted to those who contributed examples to Mr. Teulon-Porter: Mr. Cefni Barnett, of Newport Museum, the Rev. Canon E. L. Cochrane, the Very Rev. H. Lewis Crosby, Mr. G. D. Cuznor, of Bridgewater Museum, the Rev. Gordon Hulkin, Professor J. Raftery, of the National Museum of Ireland, Mr. Warwick Smith, of Peterborough Museum, Mr. J. Summerson, of Sir John Soane's Museum, and Mr. L. E. Tanner, of the Muniment Room, Westminster Abbey.

<sup>2</sup> In Germany there is a custom called *Richfest*, in which, on the completion of a building, before the tiles are put on, a garland is tied to the roof-tree, no doubt as a substitute for the live cock or other animal, which was sacrificed on the summit of the new roof in earlier days.

<sup>3</sup> It does not seem likely that the fact that St. Michael Royal is the church where Dick Whittington is reputed to be buried has any connexion with this dried cat, although it is just possible that a knowledge of the old story may have suggested a revival of the practice of sacrificing a cat to arrest plague or pestilence.



# A SPECTROPHOTOMETER FOR MEASUREMENT OF SKIN COLOUR\*

by

J. S. WEINER, M.A., M.SC., PH.D., M.R.C.S.

*Anthropological Laboratory, Department of Human Anatomy, University of Oxford*

**253** The Hardy photo-electric recording spectrophotometer was used by Edwards and Duntley (1939) for the measurement and analysis of the colour properties of the human skin. By means of this instrument they were able to present the complete reflectance spectrophotometric curves of the skin over the range of the visible spectrum from 4000 to 7000 Å°. From these they determined over the general body surface the distribution not only of melanin but, as they claim, also of carotene and of reduced haemoglobin and oxyhaemoglobin. They identified the presence of an additional pigment contributing to human skin colour, namely, 'melanoid,' related to melanin and absorbing strongly in the violet part of the spectrum. From these reflectance curves it was possible to specify the colour of the skin in terms of its brightness, dominant wavelength and degree of purity (Hardy, 1936; Wright, 1944). To illustrate the method, Edwards and Duntley presented some comparisons between the sexes and between individuals of different racial groups. Earlier, Sheard and his colleagues (1926; 1929), using a less satisfactory instrument of a different type, obtained reflectance curves from which colorimetric specifications were derived, but using the now superseded O.S.A. system of 1922 (Troland, 1922). Their main findings are in general accord with those of Edwards and Duntley (1939).

One of the most pressing and long-standing requirements in physical anthropology is for an objective method of skin-colour measurement. That reflectance spectrophotometry of the skin has not come into general use is no doubt due to the fact that the elaborate Hardy instrument of the Massachusetts Institute of Technology was (and probably still is) quite unique and that no simpler apparatus for general laboratory or field work has been forthcoming, although a relatively simple photo-electric reflectance colorimeter for use in the paint industry was described by Bolton and Williams as long ago as 1937. The portable trichromatic colorimeter suggested still earlier by Williams (1933) would not give a performance comparable to that of the Hardy type of instrument or to that of Bolton and Williams.

The only instrumental method which has been used to any extent in anthropology is the Bradley colour top, whereby arbitrarily chosen colours are blended by whirling and then matched visually against the skin. However, this instrument has failed to secure general adoption. Objections to it include the uncertainty of the standardization of the colours and the difficulty of equating the proportion of the colours in the blend to actual skin pigments. Using

\* With three text figures

this instrument Davenport (1910, 1913) was able, nevertheless, to make his well-known contribution to the genetics of skin colour. The Bradley colour top whose use was explored by Todd (1921), Herskovits (1926) and others is now apparently unobtainable.

A photo-electric spectrophotometer—the 'EEL' Reflectance Spectrophotometer made by Evans Electroselenium, Ltd., Harlow, Essex—has recently been put on the market, intended for industry, which (judging from the tests reported below) gives spectral reflectance values of the human skin comparable to those of the Hardy instrument and makes possible also the specification of the colorimetric properties of the skin. In view of its portability and simplicity it would appear to be suitable for anthropological fieldwork. The instrument appears in most respects to be similar to that of Bolton and Williams (1937).

In this instrument (fig. 1) the light source is a six-volt lamp; the beam is focused by a lens and passes through a



(a) (b)  
FIG. 1. THE 'EEL' REFLECTANCE SPECTROPHOTOMETER  
(a) Applicator head; (b) galvanometer  
Photograph: Davey Reynolds

heat-absorbing glass. A light spot of 3/8 inch diameter strikes the skin at an angle of 45° and the amount of light reflected vertically from the surface is received by the photo-cell mounted above the aperture. The light beam can be interrupted, in the present instrument, by any one of nine narrow-band spectrum filters (Ilford, Nos. 601 to 609) mounted on a wheel which enables each filter to be used rapidly in turn. The optical system and the photo-cell are housed in a movable head (fig. 1a) which can be applied quite easily to the skin surface in most regions. By marking of the skin, positioning can be made quite exact. The reflectance at the nine dominant wavelengths of the filters is measured relative to that of a white standard



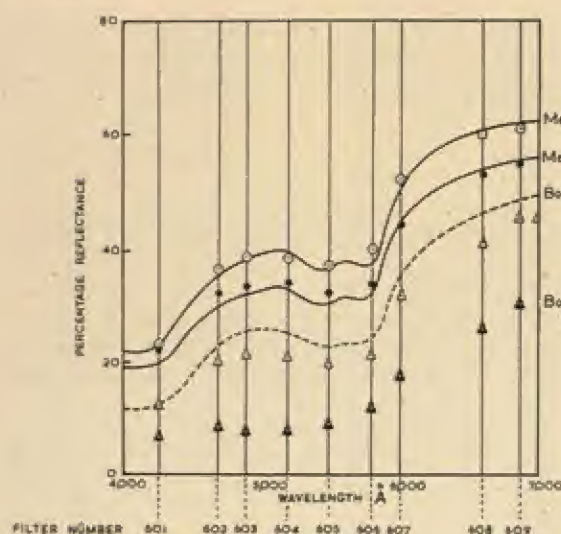


FIG. 2

The three curves are for European males, from the paper by Edwards and Duntley (1939). The three uppermost series of points are respectively for the medial side of the arm (top), the medial side of the calf, and the back of the hand of a European male, and have been obtained with the EEL instrument. The lowermost series is for the back of the hand of an Indian (Tamil) male.

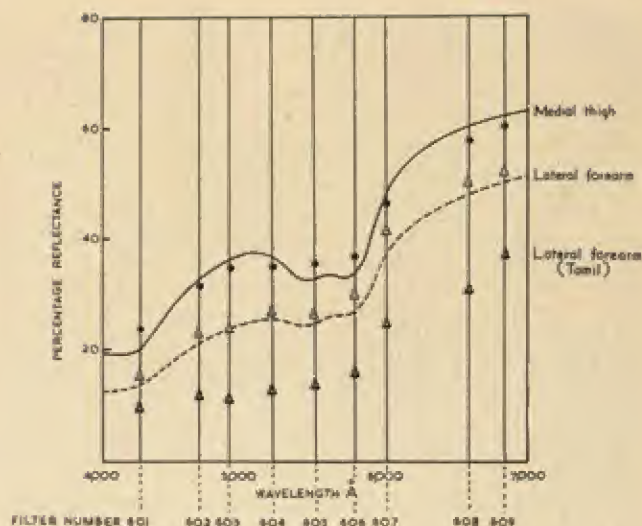


FIG. 3

The two curves are for European males, from the paper by Edwards and Duntley (1939). The two uppermost series of points are for the medial side of the thigh (top) and the lateral side of the forearm of a European male, and have been obtained with the EEL instrument. The lowermost series is for the lateral side of the forearm of an Indian (Tamil) male.

(magnesium carbonate block) from readings of the galvanometer (fig. 1b).

The instrument thus gives an abbreviated spectrophotometric curve of light reflected from the skin. In figs. 2 and 3 the values obtained on a European male by means of this instrument are compared with some of the curves given by Edwards and Duntley (1939). In the five regions shown, the values of the nine wavelengths of the Evans instrument can be seen to follow those of the Hardy instrument quite closely despite the fact that the wave band in the latter instrument is much narrower than is possible with optical filters. The difference between exposed and relatively unexposed parts of the skin is revealed clearly (cf. medial surface of the arm as compared with back of hand (fig. 2)). Repeated readings agree closely; duplicate observations made on the inner thigh region were found to differ by less than 1 per cent. The greater degree of pigmentation of the Indian (Tamil) as compared with the European subjects is clearly illustrated in figs. 2 and 3.

The instrument does not, of course, permit the detailed analysis of skin pigments possible with the Hardy instrument. Despite their approximate nature, the reflection curves appear, from a few calculations, to yield values for dominant wavelengths and purity of an order comparable to that given by Edwards and Duntley (1939). These calculations are tedious and the accuracy of the results is difficult to assess. In the Evans spectrophotometer, however, it is possible to obtain directly the measurements needed for colour-specification. The nine-filter wheel is removed and a three-filter ('tri-stimulus') wheel is substituted. With this, not only can the percentage brightness be obtained directly, but the reflectance values for the

three filters yield the coefficients from which the other colour values can be derived. The percentage brightness is probably the most useful and reliable of these values. Some representative figures obtained on a second European subject may be quoted:

Inner arm: Percentage reflection 49.0; dominant wavelength 0.58; purity 10 per cent.

Back of hand: Percentage reflection 31.0; dominant wavelength 0.59; purity 19 per cent.

It is thus possible by the use of standard filters to make comparable measurements objectively not only of the spectral-reflection curve of the skin, but also of its optical characters. It needs only about 10 minutes to make observations on one area of the skin.

The instrument can be driven from the mains or from six-volt batteries and is compact and transportable.

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## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

### Blood Group Research: Nuffield Foundation Grant to the Royal Anthropological Institute

**254** The symposium on Blood Groups and Anthropology on 17 March, 1951, organized by the Royal Anthropological Institute and reported in *MAN*, 1951, 157-164, served to focus attention on the need for co-ordination of the work now being done in a relatively new field of research. Arising from this symposium the Institute appointed a Blood Group Committee, and a grant of £14,000 has been made by the Nuffield Foundation to establish a research centre, which will be known as the Nuffield Blood Group Centre of the Royal Anthropological Institute.

The new centre will classify the large and rapidly growing body of data on the distribution of the human blood groups throughout the world, assess it statistically and make the results available to anthropologists and other research workers. It will also carry out and stimulate work in this country and abroad where this is desirable in order to follow important clues or to fill gaps in the world picture, and in particular will continue work now in progress on the classification of British blood-donors.

There is good evidence that the distribution of certain diseases is related to the race of the patients, and some evidence that many other diseases may be so affected. The blood groups are among the few clear-cut characters which show a wide variation in frequency even in a country as small as Great Britain, and classified data about them will be a necessity to anyone interested in showing up any anthropological element in the aetiology of any disease. The new centre, while primarily catering for anthropologists, may thus make an important contribution to medical research.

The centre will be at the Royal Anthropological Institute, 21, Bedford Square, W.C.1, and it is hoped that it will be in operation on 1 January, 1952. It will greatly facilitate the work if persons publishing or having published papers on the frequencies of blood groups and other genetical factors in particular populations will send offprints to the centre. Dr. A. E. Mourant, who is Hon. Secretary of the Institute Committee which is responsible for administering the centre, will also (by agreement with the Medical Research Council) be Hon. Director of the Centre, and offprints will be gratefully received and acknowledged by him.

## SHORTER NOTES

**U.N.E.S.C.O.'s New Statement on Race.** Cf. *MAN*, 1950, 220 and 1951, 28-32, 93-6, 101, 122, 151, 180, 200, 201 and 229

**255** The second meeting convened by U.N.E.S.C.O. to consider problems of race took place from 4 to 8 June this year. Readers of *MAN* will not need reminding that it was the publication in these columns of criticisms, by British and other anthropologists, of the original Statement which led to the decision to summon a new panel, confined this time to authorities on the physical aspects of race; some account of the background of the June meeting will be found in *MAN*, 1951, 101.

The panel was excellently balanced in point both of specialisms and of nationalities. Professor Ashley Montagu was included as a representative of the panel which drew up the 1950 Statement with which his name is associated. Of the new panel, Professor Vallois was elected chairman and Professor Dunn rapporteur. The full membership was as follows:

Professor R. A. M. Bergman, Professor of Anthropology, Netherlands Anthropological Society, Amsterdam; Professor Gunnar Dahlberg, Director, State Institute for Human Genetics and Race Biology, University of Uppsala, Sweden; Professor L. C. Dunn, Department of Zoology, Columbia University, New York; Professor J. B. S. Haldane, F.R.S., Department of Biometry, University College, London; Professor M. F. Ashley Montagu, Department of Anthropology, Rutgers University, New Brunswick, N.J.; Dr. A. E. Mourant, Director, Blood Group Reference Laboratory, Lister Institute, London; Professor Hans Nachtsheim, Institut für Genetik, Freie Universität, Berlin; Dr. Eugène Schreider, Laboratoire d'Anthropologie Physique, Institut de Paléontologie Humaine, Paris; Professor Harry L. Shapiro, Chairman, Department of Anthropology, American Museum of Natural History, New York; Dr. J. C. Trevor, Faculty of Archaeology and Anthropology, University of Cambridge; Dr. Henri V. Vallois, Professeur au Muséum d'Histoire naturelle, Directeur du Musée de l'Homme, Paris; Professor S. Zuckerman, F.R.S., Department of Anatomy, Medical School, University of Birmingham.

In addition, Dr. Julian Huxley, F.R.S., and Professor T. Dobzhansky (Department of Zoology, Columbia University, New York) contributed to the wording of the draft.

It is a mark of the wisdom and circumspection with which U.N.E.S.C.O. is now handling this subject that no attempt was made to produce a final Statement at the June meeting. On the contrary, the resulting document was intended only as a provisional draft, subject to further amendment both by members of the panel themselves and in the light of comments received from other anthropologists, biologists and geneticists, and from learned societies, throughout the world. It is intended to publish the final version during 1952 together with appendices containing the comments and discussions to which its preparation gives rise.

The Hon. Editor of *MAN* had received permission to publish both the provisional text itself (incorporating all amendments approved up to 15 October) and Professor Dunn's excellent, and less formal, Report on the proceedings. He has, however, been asked, just before going to press with this issue, not to publish the draft Statement at this stage, and has of course acceded to this request, as also, he understands, has the Editor of the *American Journal of Physical Anthropology*. This request has been due, it would appear, to an intervention by the Mass Communications Department of U.N.E.S.C.O. (responsible for press relations) for reasons which do not, to the Hon. Editor, seem wholly adequate.

An up-to-date version will be printed in *MAN* as soon as authority is given by U.N.E.S.C.O. It is to be hoped that the appropriate Departments will see the wisdom of getting all the controversy—public as well as private—over before a 'definitive' Statement is issued. In the meantime, Professor Dunn's Report, printed below, gives a clear impression of the great progress which was made at the June meeting towards the preparation of a Statement which shall finally dissociate anthropological science from racialism in the eyes of the world.

The Hon. Editor takes the opportunity of repeating the



suggestion, now addressed specifically to the organizers of the Vienna session in September, 1952, that the Statement in its final form should be given full consideration, and if possible approval, by the International Congress of Anthropological and Ethnological Sciences.

*Professor Dunn's Report*

The reasons for convening a second meeting of experts to discuss the concept of race were chiefly these:

Race is a question of interest to many different kinds of people, not only to the public at large, but to sociologists, anthropologists and biologists, especially those dealing with problems of genetics. At the first discussion on the problem of race, it was chiefly sociologists who gave their opinions and framed the Statement on Race. That Statement had a good effect, but it did not carry the authority of just those groups within whose special province fall the biological problems of race, namely the physical anthropologists and geneticists. Secondly, the first Statement did not, in all its details, carry conviction of these groups and, because of this, it was not supported by many authorities in these two fields.

In convening a new conference, a special effort was made to have better representation from these latter fields than had been possible before. The members of the new conference were all biologists, in the wider sense of the word, that is, that it included physical anthropologists (Ashley Montagu, Bergman, Trevor, Schneider, Shapiro, Vallois, Zuckerman), geneticists (Dahlberg, Dunn, Haldane, Nachtsheim) and one serological anthropologist, specialist in the distribution of human blood groups (Mourant).

The discussions were marked by a very evident desire, on the part of all, to reach common ground. In this there is no question that success was attained.

In general, the chief conclusions of the first Statement were sustained, but with differences in emphasis and with some important deletions. There was no delay or hesitation or lack of unanimity in reaching the primary conclusion that there were no scientific grounds whatever for the racialist position regarding purity of race and the hierarchy of inferior and superior races to which this leads.

We agreed that all races were mixed and that intra-racial variability in most biological characters was as great as, if not greater than, inter-racial variability.

We agreed that races had reached their present states by the operation of evolutionary factors by which different proportions of similar hereditary elements (genes) had become characteristic of different partially separated groups. The source of these elements seemed to all of us to be the variability which arises by random mutation, and the isolating factors bringing about racial differentiation by preventing intermingling of groups with different mutations, chiefly geographical for the main groups such as African, European and Asiatic.

Man, we recognized, is distinguished as much by his culture as by his biology, and it was clear to all of us that many of the factors leading to the formation of minor races of men have been cultural. Anything that tends to prevent free exchange of genes amongst groups is a potential race-making factor and these partial barriers may be religious, social and linguistic, as well as geographical.

We were careful to avoid dogmatic definitions of race, since, as a product of evolutionary factors, it is a dynamic rather than a static concept. We were equally careful to avoid saying that, because races were all variable and many of them graded into each other, therefore races did not exist. The physical anthropologists and the man in the street both know that races exist: the former from the scientifically recognizable and measurable congeries of traits which he uses in classifying the varieties of man; the latter from the immediate evidence of his senses when he sees an African, a European, an Asiatic and an American Indian together.

We had no difficulty in agreeing that no evidence of differences in innate mental ability between different racial groups has been adduced, but that here too intra-racial variability is at least as great as

inter-racial variability. We agreed that psychological traits could not be used in classifying races, nor could they serve as parts of racial descriptions.

We were fortunate in having as members of our conference several scientists who had made special studies of the results of inter-marriage between members of different races. This meant that our conclusion that race mixture in general did not lead to disadvantageous results was based on actual experience as well as upon study of the literature. Many of our members thought it quite likely that hybridization of different races could lead to biologically advantageous results, although there was insufficient evidence to support any conclusion.

Since 'race,' as a word, has become coloured by its misuse in connexion with national, linguistic and religious differences, and by its deliberate abuse by racialists, we tried to find a new word to express the same meaning of a biologically differentiated group. On this we did not succeed, but agreed to reserve 'race' as the word to be used for anthropological classification of groups showing definite combinations of physical (including physiological) traits in characteristic proportions.

We also tried hard, but again we failed, to reach some general statement about the inborn nature of man with respect to his behaviour toward his fellows. It is obvious that members of a group show co-operative or associative behaviour towards each other, while members of different groups may show aggressive behaviour toward each other—and both of these attitudes may occur within the same individual. We recognized that the understanding of the psychological origin of race prejudice was an important problem which called for further study.

Nevertheless, having regard to the limitations of our present knowledge, all of us believed that the biological differences found amongst human racial groups can in no case justify the views of racial inequality which have been based on ignorance and prejudice, and that all of the differences which we know can well be disregarded for all ethical human purposes.

## International Congress of Anthropological and Ethnological Sciences, Fourth Session, Vienna, 1952

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The following is the text of the First Circular, issued in July, 1951, of the Fourth International Congress of Anthropological and Ethnological Sciences:

At the third session of the Congress, held in Brussels in August, 1948, it was decided that the next Congress should be held in Vienna. Accordingly, the Fourth Congress will assemble there from September 1 to 8, 1952. The Austrian Government and the scientific institutions and societies in Vienna have guaranteed full support and collaboration.

The Austrian Executive Committee has been constituted as follows: President, Rev. Father Professor Wilhelm Schmidt (Anthropos Institute); Vice-President, Professor Robert Heine-Geldern (Vienna University); Secretaries, Professors Wilhelm Koppers and Josef Weninger (Vienna University); Assistant Secretary, Dr. Anna Hohenwart-Gerlachstein; Treasurer, Dr. Walter Graf; Members, Professor Robert Bleichsteiner, Professor Wilhelm Cermak, Dr. Wilhelm Ehgartner, Professor Arthur Haberlandt, Professor Josef Haeckel, Professor Franz Hančar, Dr. Helga Pacher, Professor Richard Pittioni, Professor Leopold Schmidt, Dr. Alexander Slawik, Dr. Margarete Weninger, Professor Dominik Josef Woelfel (all from Vienna University), Professor Wolfgang Amschler (University of Agriculture, Vienna), Dr. Etta Becker-Donner (Museum of Ethnology, Vienna), Dr. Herbert Mitscha-Märheim (President of Anthropological Society, Vienna), Dr. Karl Krenn (Department of Prehistory, Museum of Natural History, Vienna), Dr. Robert Rutil (Department of Anthropology, Museum of Natural History, Vienna), Rev. Father Professor Paul Schebesta (St. Gabriel Seminary for Foreign Missions, near Vienna), and Dr. Josef Wastl (Vienna). All correspondence should be addressed to the Secretary, Professor Wilhelm Koppers, Institut für Völkerkunde, Neue Hofburg, Corps de Logis, Vienna I., Austria.

The membership fee is 200 Austrian schillings or \$8. It covers



various scientific tours and social functions. All members will receive a copy of the Proceedings. A member may register up to two members of his family as associates. The fee for Associate membership will be 100 schillings or \$4. They may attend the meetings, excursions and receptions, but may not speak or vote, and they will not receive the Proceedings.

Members who intend to submit papers may from now on send in the titles to the Secretary. Except in special cases, the time allotted to every paper will be twenty minutes, plus another ten minutes for discussion. Members should say whether they will require a projector and indicate the size of their slides or films. Apart from anthropological and ethnological subjects in the strict sense, papers may deal with questions of applied ethnology, demography, sociology, psychology (as referring to ethnological problems), science of religion, linguistics, folklore, prehistory, palaeo-ethnology, origin and distribution of cultivated plants and domesticated animals.

In due course further circulars will be issued giving full information as to the sections of the Congress, the easiest way of remitting fees, the possibilities of accommodation in Vienna, etc. By way of preliminary information, the prices of rooms in Vienna are at present: first-class hotels, single rooms from 50 sch., double rooms from 60 sch., second-class hotels, single from 25 sch., double from 38 sch., students' hostels, single from 6 to 10 sch., double from 12 to 20 sch.

The Austrian Committee cordially invites anthropologists, ethnologists, sociologists, folklorists, linguists, prehistorians and archaeologists of all nations to attend the Congress. It will do its utmost to make it, from both the scientific and social points of view, worthy of the previous sessions, in London (1934), Copenhagen (1938) and Brussels (1948).

Members who will attend the Congress as delegates of govern-

ments, universities, scientific institutions or scientific societies are requested to bring this to the knowledge of the secretary.

On behalf of the Officers

of the Permanent Council:

JOHN L. MYRES, H. J. FLEURÉ,

KAJ BIRKET-SMITH, FRANS M. OLBRECHTS,

On behalf of the Austrian

Executive Committee:

WILHELM SCHMIDT, ROBERT HEINE-GELDERN,

WILHELM KOPPERS, JOSEF WENINGER

## Current Work of the Edinburgh School of Social Anthropology. Communicated by Dr. K. L. Little, Head of the

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Department

The Department of Social Anthropology, University of Edinburgh, is carrying out a number of field investigations with a view to testing the application of anthropological methods to the study of modern society and social institutions. Work in progress includes studies of local groups in the Scottish Lowlands, the Hebrides, the Shetland Islands, and Sierra Leone, West Africa, and five studies of different aspects of race relations in Britain.

## Lectures at the Horniman Museum

Among lectures of interest to anthropologists in the current series at the Horniman Museum, Forest Hill, London, S.E.23, are: 'The Malay Races down the Centuries,' by Sir Richard Winstedt (24 November); 'Stone Age Tools,' by Mr. A. D. Lacaille (1 December); and 'Nature and Man in the Island of Bali,' by Miss Beryl de Zoete (8 December). All are at 3.30 p.m. and are illustrated.

# REVIEWS

## GENERAL

**Der Mensch geringer Naturbeherrschung.** By Richard Thurnwald. Berlin (de Gruyter). 1950. Pp. 157, 22 plates, map. Price DM. 4.80

This is a closely written outline of the earlier stages of human development, embodying the author's well tried system of exposition, with no loss of vigour and clarity. He takes account of recent crises especially as affecting German outlook and demanding a restatement of anthropological theory and method, based on observation and recent criticism. There is no more room in the humanities than in physics for obsolete theories; and there is much more to clear away.

Archæology and ethnography supplement each other, when account is taken of resemblances of technique among the crafts and material arts, even of widely separated peoples. In this way 'Phases' of culture may be constituted, across the many 'Variants' of humanity and its outlook on nature; and they may be found to be related to 'phases' in folk psychology.

Only the broad outlines are presented here, and main connexions and characteristics are inevitably emphasized. But the broad outlines must be based on individual examples, which alone have scientific validity.

The material in this summary volume is mainly derived from the author's well-known articles in Ebert's *Reallexikon der Vorgeschichte* and the five volumes of his *Menschlichen Gesellschaft*: 1931-1933. There is a bibliography of references to his other publications, and the lively and ingenious diagrams are the sequel to those in his earlier works. The book is therefore a welcome supplement.

JOHN L. MYRES

**Race Relations: The Interaction of Ethnic and Racial Groups.**

By Brewton Berry. Boston (Houghton Mifflin), 1951. Pp.

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xii, 487. Price \$4.75

The author describes this as an elementary textbook for American college students; he defines his field sensibly, saying 'properly speaking, races are zoological categories, and we

do not have social relations between zoological categories . . . race relations are not so much the relations that exist between the members of different races as between people conscious of those differences' (p. x). Thus it tends to become a book about culture contact and immigration, assembling well presented material about Chinese Americans, Negro Americans, the 'White Australia' policy, Magyar and Roumanian, Tungus and Cossack—to give but a few examples. The attempt to get everything in deprives it of any *ausgangspunkt*; had the section on prejudice been more fully and systematically treated and a few examples analysed—not only on the sociological plane, but indicating the complexity of contact and conflict—the reader might have been provided with a critical apparatus to deal with the other cases if he should ever have to. Easy to read and profusely (though sometimes childishly) illustrated, this will not be found to meet the rather different requirements of race relations courses in British universities. MICHAEL BANTON

**The Sociology of Religion.** By Joachim Wach. Edited by Karl Mannheim. London (Kegan Paul), 1947. Pp. 429. Price £1 10s.

This is a monumental work embodying the results of vast scholarship. It is described as a sketch and the presentation is indeed concentrated. But the book deals with a number of the most important problems in the sociological study of religion.

The first part, described as methodological prolegomena, gives not only the method of approach to the problems but also some of the author's views on the nature of religious experience and on the sociological functions of religion. From the outset the author accepts religion as having an integrating function for social life. His personal position is that religious experience has an objective character independent of the personality having that experience. The second and much larger part of the book discusses the relation between religious organization and society. The author is concerned with problems of religion in relation to 'natural' groups of society such as family and kin groups, local and national groups; in relation



to social differentiation, in relation to the political authority of the state. He gives also an interesting analysis of the internal organization of religious associations, and especially of the principles of exercise of authority. The treatment is largely historical and comparative.

The author draws on the conceptual framework of Max Weber but includes a larger range, especially primitive material which Weber did not touch. In line with his personal approach, however, he regards sociology as unable to give a complete understanding of religion, not because of the need for further supplementing of theory by sciences such as psychology, but because of the impossibility of revealing 'the nature and essence of religion' by sociological study. Anti-historicist, he argues for the retention of the true and the false as categories of judgment in a revitalizing discipline of hermeneutics, the theory of interpretation.

The book is full of suggestive generalizations and hypotheses, which it is impossible to summarize in a review. Because of its very wide scope, however, and the enormous number of footnotes, it is difficult to handle. In particular it does on the whole fail to provide that intensive analysis of a religious system in relation to other aspects of the social structure which the best anthropological monographs give. But it amply fulfils the author's intention to serve as a basis for lectures in class.

RAYMOND FIRTH

**Sociologie et Psychanalyse.** By Roger Bastide. Paris (Presses Universitaires de France), 1950. Pp. viii, 290. Price 600 fr.

**262** The sociology of Freud has been long discredited by his too ready acceptance of concepts that were being outmoded at the time he was writing. Some followers of the master, for example Kardiner, have attempted to refine the tools he developed and to use them for social analysis with anthropological data. On the face of it the whole psycho-analytical battery of tools, the super-ego, the ego, the id, the force of libido, the Oedipus complex, the mechanisms of transference and projection, etc., should be of the utmost value to a fieldworker.

The present book takes the conceptual framework of Freudian social psychology, submits it to a careful analysis, and compares it with the sociological standpoint as developed in the early years of this century by Durkheim and others. Later chapters continue this analysis, comparing the influence of psycho-analytical literature on more modern sociologists, on later marxian thought, and on anthropological work, principally as shown by the writings of Benedict, Mead, Malinowski and Kardiner. These chapters are valuable critical commentaries on trends, as well as on the development of methods for studying societies and on the theoretical understanding of society in general in the present century.

The results of this study are summed up in the two final chapters. The necessity for the social research worker, whether anthropologist, social psychologist or sociologist, to know much about himself, his prejudices, and his unconscious in order that he may see the activities of others, is emphasized; it is true to say that the dangers inherent in a corps of psycho-analysed anthropologists are also recognized. Since social facts are expressed through the individuals who manifest them, it is equally important to know the processes at work within the individual. Bastide believes that the theoretical framework for understanding the individual presented by Freud should open up new worlds of knowledge in both sociology and anthropology. In his summing-up he argues strongly for the pure doctrine of Freud, as against the recent modifications of Fromm and Horney.

For the anthropologist, this volume of the *Bibliothèque de Sociologie contemporaine* is important whether one can agree with the author or not. It brings together two fields of thought, each of which has contributed to the theory of modern social anthropology.

J. M. MOGEY

**Totem and Taboo.** By Sigmund Freud, translated by James Strachey. London (Routledge & Kegan Paul), 1950. Pp. xi, 172. Price 14s.

**263** It has been claimed that Freud's impact on psychology was comparable to that of Newton or Einstein upon physics. This is, if anything, an understatement, for not only did he initiate a profound scientific revolution, but he also enriched the insight

of countless individuals, and influenced, among others, the wide and varied fields of art, social study and administration.

It is unfortunate therefore that Freud's main contribution to anthropology should be generally considered as brilliant but misguided guesswork. The word 'misguided' is used advisedly, for, writing in 1912 and 1913, he relied on early ethnographers whose material was unsuitable for the development of psycho-social hypotheses. (It is interesting to speculate on how he would have treated the detailed and coherent material presented in modern field studies).

Nevertheless, although we may consider such an idea as group marriage as incorrect, or at least unverifiable, we cannot simply dismiss *Totem and Taboo* as an aberration or curiosity. We may reject the historical interpretations, and distrust the over-emphasis on unconscious motivation, but we cannot ignore the intimate link between the individual and his community. Consequently, if we accept, as we must, the existence of the unconscious, we have also to recognize the interplay between the life of the community and the individual unconscious. If Freud, with the ruthless single-mindedness of the pioneer, over-stressed the unconscious determinants of social behaviour, we must at least be grateful to him for being the first to make them an object of study.

This new edition is very pleasantly produced, and translated with the complete competence and understanding which we always expect from Mr. James Strachey.

ADAM CURLE

**Psycho-analysis and the Social Sciences, Vol. III.** Edited by Géza Róheim. New York (Internat. Univ. Press), 1951. Pp. 312, illus. Price \$7.50

**264** Psycho-analysis is partly a faith and partly a science. This series of essays by the faithful demonstrates clearly the limitations of this method of interpreting social phenomena. The five sections, anthropology, folklore, religion, literature and sociology, add little to knowledge. A certain interest, however, is attached to the use of psycho-analytical words as thinking devices in these fields.

J. M. MOGEY

**The Birth of Civilization in the Near East.** By Henri Frankfort. London (Williams & Norgate), 1951. Pp. 126, 37 illus. Price 16s.

**265** It was high time for a fresh survey of the earliest traces of civilization—which Frankfort distinguishes clearly from 'culture.' It is the study of man as a civil member of a society; and this conveniently defines his present subject. He does not attempt to answer the question of origins, which he thinks no historian can answer. But in each period there is a 'general form of the forms of thought'; and this, however arduously, it is the historian's business to discover.

The 'form' of any civilization is best appreciated by comparison with some other such; and for such comparison Egypt and Mesopotamia are ready to hand. In both, much depends on the physical surroundings, and especially on the rivers which sustain the earliest settlements. Probably few people realize how much has been discovered in the last generation about the simplest modes of life, especially in Mesopotamia. Some of them present hardly more than a few families; but the bond seems to be that of locality rather than of kinship. And this accords with the earliest known settlement sites such as Hassuna near Mosul. In Egypt such settlements are obliterated by cultivation or the spread of Nile sediment.

Before commenting on these primitive data Frankfort examines the theories of Spengler and Toynbee, and confronts them with inconvenient facts. Both are misled by their preoccupation with decay, prejudice against ideas of progress and of humanity as a whole, and confusion of scientific with historical problems and methods. Spengler's 'Egyptian soul' is 'totally at variance with the evidence' (p. 24); his 'Urmensch' is the Greek or the Aryan Indian (p. 21). Toynbee too (p. 23) 'invests certain images which he uses with a spurious reality': they are 'ostensible similes' and do not 'reflect historical situations.' Toynbee's notion of progress (p. 25), already devastated by Collingwood, is an uncritical generalization, which does not escape from nineteenth-century preconceptions; his 'experience' derives from his classical training, and he postulates for Egypt a 'universal church created for an internal proletariat.' Here every term is misapplied: for we have the Egyptian people's own views about their kings and the state of the kingdom,



at all the principal periods. They trusted their political 'shepherds' and for whole dynasties they were not disappointed: for in Egypt a god ruled. So too in Mesopotamia, all were servants of the 'city'; and it was only when some other 'city', or outer barbarians who had no 'city' at all, prevailed like a flood or an earthquake, that their civilization failed them. It is to examine these 'forms' of civilization that Frankfort devotes himself, setting out a large mass of details in a clear and coherent scheme: a 'form,' in his sense, is a certain consistency in orientation, a cultural style (p. 32). Some current misapprehensions have to be cleared up. Irrigation, for instance, may be earlier than the domestication of seed plants—this was recorded in 1822 by Burckhardt in Nubia (p. 37)—and domestic animals than crops. The earliest grain-cultivators were migratory, and propagated their 'civilization' thus. Continuity is hardly to be separated from the first temples (p. 47).

Not in the earliest Mesopotamian cities only, but far on into historic times, there was no hard division between urban and rural. The ordinary member of the community was himself a cultivator on the city's land, whatever his personal skill as a craftsman using materials provided from the temple stores; from which also he drew his reserves of food.

The 'city' is not a 'mere conglomeration of people' (p. 37). Even in prehistoric times the 'communal life became civic life.' Of this the symbol was the *ziggurat*, the temple mound raised out of reach of flood and giving access to the god; the instrument of social cohesion was the *script*; and the economic unit the temple community with its 'elders,' and—for emergencies—its 'great man,' *lugal* or *ensi*. Here too, institutions are revealed to us full-grown, and encumbered with abuses and remedies for them; but the ultimate power lay with the common people, to whose support the great administrators appeal.

In Egypt, where the physical conditions were different, it was the king, not the city, that was sovereign. Indeed there were no real cities; even Memphis was a court, not a capital. And here too the system is revealed full-grown, and of amazing stability and efficiency.

A more detailed appendix deals with evidence for Mesopotamian influence on early Egypt.

The numerous illustrations include many new subjects, as well as some that are familiar.

JOHN L. MYRES

**Society: An Introductory Analysis.** By R. M. Elver and Charles H. Page. London (Macmillan), 1950. Pp. xvii, 697, diagrams, biblio., index. Price £1 5s.

In this English edition of a well-known American textbook, the authors have added a foreword on the question often heard in this country, 'What is sociology about, anyway?' The six hundred or so pages should provide a convincing answer. The basic theme is given as the study of social relationships, of the

forms, varieties, patterns and systems of such relations in western society. It is thus not surprising to find that the classes of social relations distinguished owe much to the sociology of Georg Simmel and his followers, principally von Weise and Becker, although in other ways this book could hardly be described as 'formal' sociology.

The traditions of American university teaching demand a longish and careful introduction on the terms and concepts which are to be employed; then follows the main body of the text, in 25 chapters, each giving the gist of relevant literature with a paragraph or two of commentary by the authors. For textbooks of sociology the main sections are now almost stereotyped. A part early in the book, entitled 'Society and Environment,' gives some account of the various geographical schools beginning with Le Play, extending to Ellsworth Huntington and writers on geopolitics and concluding with a fair summing-up of the relations between geography and civilization. Book II, 'Social Structure,' begins with a discussion of the codes of behaviour and social control associated with custom, law, morals and religion, but the major part is devoted to social groups and their characteristics. These groups include the family, the community, the region or neighbourhood, social class and caste, voluntary organizations and so on. The section normally headed 'Social Institutions' is here called the 'Great Associations,' political, economic, and cultural. Book III deals with social change, a subject of absorbing interest to Americans. Problems of demography, of technological innovation and of cultural adaptation are presented in this part.

Throughout, the text contains copious references to literature in books and articles, drawing on American and Continental sources. English writers are mentioned, but references to their work are often general. The reading list refers to modern American and Continental books and to the older classical studies of English writers. No contemporary English university textbook covers this ground, for there is no subject called sociology at most of our universities. Equally most of the specialized studies referred to are American, for without the stimulus of a university course research is difficult. The absence of a discussion of theories of society or of aspects of social living follows from the sub-title. The English student of anthropology will be impressed by the range and variety of sources, by the care with which the essential facts have been abstracted and deployed and by the broad sweep of the topics discussed. However, no section goes into any great detail, in many cases there is a dearth of information on how the facts were collected, and it will not be surprising if the hypothetical English student of anthropology prefers in the long run to read parts of the field in the monographs and form his own compendium of fact and opinion in our traditional and rather haphazard way. Even so, for those who are searching for bearings in this rather uncharted and unclear field of study, this text will certainly prove an admirable guide. J. M. MOGEY

## ASIA

**Contribution to the Prehistory of Mongolia.** By John Maringer. *Reports of the Sino-Swedish Expedition to the North-Western Provinces of China, Publication 34.* Stockholm, 1950. Pp. xii, 216, 40 plates. Price Sw. cr. 72

When Folke Bergman, the archaeologist of Sven Hedin's expedition to the north-western provinces of China, died in 1946 he had not yet prepared the publication of the immense bulk of prehistoric material—chiefly stone implements and stone-working wastage—collected on the route of the expedition. This material, at present stored along with Bergman's minute catalogue of the pieces in the Museum of Far Eastern Antiquities in Stockholm, has now been exhaustively studied by Dr. Maringer. The first part of his book deals with it site by site, combining topographical information and technical comment with detailed lists of the finds based on the catalogue; the second part consists of a discussion of the various typological categories, and the third gives the author's conclusions on the classification. The archaeologist will note with regret that no excavations were undertaken by the expedition, but he will realize also that in an area so little explored as Inner Mongolia and the Gobi desert surface material has particular importance and deserves the close study accorded it here.

Altogether 327 sites were discovered in the steppe and desert between Manchuria and Sinkiang. The majority of the stone artifacts are of microlithic type, though never in the geometric shapes familiar in India and the West, and were nearly always found associated on the surface with material of neolithic aspect. The analysis of the cores is interesting: cylindrical blade cores are very scarce in the eastern part of the area, whereas conical cores are more or less equally distributed. A third type of core, resembling in shape a small irregularly flaked celt, seems to be absent from the more western or southern regions and to be characteristic of the Mongolian-Manchurian facies of the great Asiatic spread of microlithic technique.

Another noteworthy item in the distribution tables is chipped rectangular and finger-shaped stone knives found in eastern and central Mongolia. These are not found in North China proper, whereas the polished rectangular or crescentic stone knife with circular perforations present in the North China Neolithic is absent from Inner Mongolia. Parallels to the Mongolian type are recorded from southern Siberia. The polished stone axes in most cases are comparable to types known to the north-west, though specimens resembling the squared axes of North China were also found.



Among the pottery fragments collected the most important are those with painted decoration. These are distributed over the eastern and central parts of Inner Mongolia, and some pieces from Gurnai sites (east end of the Pei Shan) 'belong to the Yang Shao stage' of the Neolithic of western China. Unfortunately Dr. Maringer does not illustrate many pieces.

Throughout his book Dr. Maringer uses the term 'neolithic' in its technological sense. He stresses the absence in his material of types directly indicating tillage. Technologically the earliest inhabitants of Inner Mongolia appear, not unexpectedly, to have stood nearer to the Neolithic than did the tribes inhabiting Outer Mongolia. But as to the crucial question of the rise of nomadism in Mongolia, whether it was produced by the degeneration of a local full Neolithic or as a response to neolithicizing influence from China or elsewhere, the material here so competently examined can give no answer. Excavations in the future may do so.

WILLIAM WATSON

**Beiträge zur physischen Anthropologie der Mongol.** By Yokoh-Yasuo. Tokyo, 1943. Pp. 106, 16 plates, 7 text figs.

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This monograph records the results of an anthropological examination of 1171 inhabitants of Mongolia. Eight hundred and fifty-three are regarded as indigenous and are collectively referred to as 'Mongols.' They are subdivided into six groups, separated by geographical and linguistic barriers. Further comparative data are supplied for a total of 318 individuals from three other groups which, 200 years ago, invaded the same general geographical area from the Chinyan mountains.

Yokoh-Yasuo follows Martin's anthropological technique and measures 21 parts of the body, for which the customary statistics are presented. Hair colour is assessed by means of the Fischer-Saller plate.

The author concludes with a verbal summary of the morphological intergroup differences revealed by his measurements.

W. BRANDT

**Archaeological Research in Indo-China, Vol. II.** By Olov R. T. Janse. Harvard-Yenching Inst. Monograph Ser., Vol. X, Cambridge Mass. (Harvard Univ. Press), 1951. Pp. viii, 300. Price \$25

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It is a tantalizing feature of Chinese archaeology of the Han Dynasty (206 B.C.-A.D. 220) that the richest excavated finds (and almost the only satisfactorily documented ones) come from two provincial centres of Chinese political and cultural influence: Lolang in Korea, a Chinese settlement made between 141 and 81 B.C., and northern Indo-China, where political control was looser though the cultural influence was almost as pervasive. The Lolang material has been admirably published by the Japanese excavators, and now, thanks to Dr. Janse, the rich harvest from tombs and kiln sites in southern Tonkin and the province of Thanh-hoà has been made available for study. The present volume is in a strict sense complementary to that published in 1947. The latter gave a general survey of the excavations and a comparison with historical data, and the present work (which regarding both text and illustrations must be read with the first volume at one's elbow) gives the final and exhaustive description of the tombs and their contents. Together these two volumes complete the account of material of Han date; a third volume now in the press (Institut Belge des Hautes Etudes Chinoises, Brussels) will record the results of excavations on the habitation site at Dong-so'n, where the effect of the earliest Chinese penetration in the region, in the third century B.C., is discernible.

Dr. Janse's presentation of the material combines clarity of reference with a most stimulating and critical attention to detail. Clear description is accompanied by discussion of local developments and comparison with analogous objects or practices in other cultures. Consequently what is inferential and interpretative is set in clear focus alongside the object or circumstance which gave rise to it, the archaeologist's cards are perpetually face upwards on the table, and the reader's mind is directed towards cultural history and not merely to formal classification. Dr. Janse's wide learning supplied the dynamism. An exquisite problem faces the archaeologist and art historian: what in this well characterized culture is owed to Chinese

intrusion (commercial and military), what is the local tradition, and what can be traced to influences from the West, ultimately from India? In discussing such things as the origin in the Indian sphere of a ceramic shape, of beads, of a style of hair dress, or in drawing on his knowledge of local tribal lore to illustrate the myth of the tree of life, the author keeps us aware of these wider aspects besides driving home an archaeological point in the strictest manner.

This volume, like the first, is beautifully printed, illustrated with 38 monochrome plates and three coloured plates of beads, as well as with 144 text figures.

WILLIAM WATSON

**Hamari Adim Jatiyan (Our Primitive Tribes).** By Akhil Venoy and Bhagavan Das Kela. Allahabad (Bharatiya Granthmala), 1950. Pp. 355, illus. Price Rs. 3. 8. **Planning for the Tribes.** By S. C. Dube. Hyderabad-Deccan (Dept. of Social Service), 1950. Pp. 30

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The authors of *Hamari Adim Jatiyan* are to be congratulated on the production of one of the first general introductions to the primitive tribes of India to appear in Hindi. The pioneer importance of such a work in the new national language is clear and merits particular attention. The book is dedicated to the late A. V. Thakkar and this dedication indicates the viewpoint of its authors as that of social uplift.

The vocabulary of technical terms used in the book seems to fall into two main parts. Those that are clearly translations into Sanskrit of terms already existing in English, and having no previous occurrence in Indian languages, and those which have occurred in Sanskrit or in the vernaculars. The choice of the latter is largely a matter of the general cultural background of the writers, whilst the construction of the former is one of the most debatable points in any expanding language. Suffice it to say that our authors have made a good show in both parts. For the rest we may admire the forceful language throughout.

Apart from useful summaries of the schemes for advancement undertaken by the Provincial Governments, and the list of various bodies and workers associated with uplift work the book provides a brief survey of the various tribes and their cultures. The discussion of the problems arising from the tribes, and of their solution, is likely to be challenged by most anthropologists, as the authors insist that social uplift need not entail any special treatment of primitive peoples, but should concentrate upon an active programme aiming at their integration into (Hindu) society.

Dr. Dube's paper, published by the Department of Social Service of the Government of Hyderabad, sets out from another viewpoint to analyse the problems of the cultural relations of the tribes. Following Elwin and Majumdar he finds that the problem varies with the many different levels of culture represented by them. He also studies the treatment of the problem in Africa, America, U.S.S.R. and finally India, and concludes with the Hyderabad Scheme formulated by Dr. C. von Fürer-Haimendorf. The resulting analysis seems far more satisfactory than that of the former authors. The approach is nowhere impractical, and there is no suggestion of an attempt to fossilize the tribes. By comparison the attitude of the authors of *Hamari Adim Jatiyan* seems rather based on a set idea of uplift to a society that is not only more advanced but also to be regarded as the ideal.

F. R. ALLCHIN

**General Code of Tribal Custom in the Jhelum District, Punjab.**

By P. N. Thapar. Lahore, 1946. Pp. xi, 146. Price Rs. 3. 13. 0

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This manual on the customary law of the various tribes and castes inhabiting a Punjab district is mainly intended for the use of executive and judicial officers, whose responsibility it is to decide disputes over the validity of marriages, the claims of divorced wives, succession to property and similar cases subject to the rule of custom. The material is arranged in the form of question and answer, with clear indication of divergencies in the customs of different population groups, and doubtful points are illustrated by numerous concrete examples in the form of court decisions. From the answers to 139 questions framed in such a way as to deal with innumerable eventualities which in practice give rise to disputes, the anthropologist can gain a fairly clear idea of kinship organization,



marriage laws and property rights of the principal communities. One only wishes that similar manuals had been compiled for other parts of India, for they would be a valuable complement to the district gazetteers and *Castes and Tribes* series.

C. VON FÜRER-HAIMENDORF

**Social Science Research in Sarawak: A Report on the Possibilities of a Social Economic Survey of Sarawak presented to the Colonial Social Science Research Council.** By E. R. Leach. London (H. M. Stat. Off.), 1950. Pp. 93, diagrams, map. Price 8s. 6d.

272

Two useful background chapters on the physical and human resources of Sarawak are followed by recommendations of essentially practical projects allocated to the Ibans, Land Dayaks, Melanau, Malays of the coast, and the tribes of the Bintulu district. The appendices are perhaps the most valuable part of the Report from the anthropological point of view and are essential for reference to any student of the area, containing, as they do, much material not recorded elsewhere. The first contains a sociological classification of the peoples of Sarawak, in which Hose's and Kennedy's widely differing classifications are set out, together with the seven wider categories which the author prefers. The second appendix deals with the basic social features, with emphasis on kinship terminologies and long-house organization. These two long appendices are followed by shorter ones on sago-cultivation and shifting dry-rice-cultivation.

Though the author has throughout kept strictly to his terms of reference he has seized the opportunity to suggest further lines of

research and if the trained anthropologists are forthcoming we can as a result of this Report look forward to a new era of fieldwork in Borneo.

J. P. MILLS

**Malay Sayings.** By C. C. Brown. London (Routledge & Kegan Paul), 1951. Pp. xvi, 274. Price 18s.

273

This pleasing collection continues a strong tradition of Anglo-Malay scholarship. Mr. Brown gives over twelve hundred Malay sayings (the 'proverbs' of earlier compilations) which he considers to be 'all the most typical Malay sayings.' He arranges them conveniently and deftly, so that in Part I, the major section of the book, the reader may find Malay expressions appropriate for the rendering of English ideas. Part II is an alphabetical index of the Malay sayings given in Part I, while an English index at the end of the book guides the reader in the use of Part I.

Mr. Brown is aware that the use of the word 'typical' may raise some questions in the reader's mind; but he believes that, outside the towns of Malaya, the sayings must still have much currency and vigour—as the terse and homely expressions of a peasantry. It would have been interesting if Mr. Brown had sketched the types of situation in which, and the frequency with which, these sayings have been employed by Malays in any place and at any period. Can they all be the counters of casual conversation or must some of them be reserved for more serious kinds of social intercourse? Are any of them the prerogative of a moralizing older generation? 'Malay Sayings in Action' would be a good future theme.

MAURICE FREEDMAN

## OCEANIA

**Women's Changing Ceremonies in Northern Australia.** By Catherine H. Berndt. *L'Homme*, No. 1. Paris, 1950. Pp. 84, maps, plates

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This monograph is the first of a number of studies which are to be published under the title of *L'Homme* by L'École Pratique des Hautes Études, 6<sup>e</sup> Section: Sciences économiques et sociales, and which will be devoted to ethnology, geography and linguistics. It should be stated at the outset that in her book Mrs. Berndt justifies the honour extended to her of inaugurating this series. She has carried out research in South and Northern Australia for many years and, in addition to articles on kinship and totemism, has devoted considerable attention to the role of women in ceremonial life. In this study she analyses in some detail women's corroborees in the west central section of the Northern Territory, and the changes which are occurring among Aborigines who have congregated around the cattle stations and who are no longer fully dependent on a hunting and collecting economy.

The ceremonies described by Mrs. Berndt are of particular interest to myself in that some of them had been taken over by women in East and South Kimberley when I visited West Australia in 1935-36. Informants told me that many of the rituals had come from Victoria River Downs, Wave Hill and the Wanciga to the south, and in my own study I suggested that such an area should provide a rich field for an investigation of women's ceremonial life. Since then Mrs. Berndt has covered much of that country in a journey which took her from Wauchope, north to Newcastle Waters, west to Wave Hill, and south through Gordon Downs and Tanami to the Granites. A comparison of the corroborees witnessed by her in the Northern Territory and by myself in the Kimberleys reveals many striking uniformities in organization, ownership, use of ritual objects, designs and songs, despite the many hundreds of miles over which the dances have been diffused. Mrs. Berndt distinguishes two main types: (a) *Tjarada*, which appear to have come originally from the eastern side of the Northern Territory near the Roper River; and (b) *Jawalju* from the 'desert' tribes in West Australia and the vicinity of Alice Springs (p. 13).

Much of the interest of Mrs. Berndt's material lies in her account of the changes taking place in ceremonial life, and in the differences of attitude to be found among the younger and older generation. She points out that in any given society there is, outside a central core of shared beliefs and attitudes, a varying range of individual

response based on differences in ritual role, sex, age, and personal experience. But in the conditions obtaining on the cattle stations, where so many Aborigines are now concentrated, individual divergence is accentuated (p. 71). Sanctions have weakened, and among some of the younger women there is scepticism about the existence of ancestral and totemic beings. The old women complain that adolescent and young married girls are frequently ignorant and uninterested, and that in so far as they participate in the rituals it is to find personal erotic satisfactions. Much of the religious significance of the ceremonies is ignored or obscured, although like other aboriginal rituals these form part of a totemic complex and are, in fact, associated with certain Ancestral Beings. For instance, much of the dancing in the *Jawalju* is a re-enactment of the travels and activities of these Beings (p. 45). But the *Jawalju*, like the *Tjarada*, includes the singing of a number of erotic and implicitly erotic songs, and Mrs. Berndt points out that, in the diffusion of these songs and dances from one group to another, there is a tendency in some cases for the features connected with love magic to be assimilated first and more quickly accepted (p. 31). Her suggestion is an interesting one and applicable to the Lunga, Djuru and Miriwun in East Kimberley, where I was unable to discover any totemic myths associated with the corroborees, and where the women stressed the erotic purpose of the songs and dances to the exclusion of any other element. In the last chapter of the book Mrs. Berndt develops this theme of changing emphasis and interpretation, and analyses those factors in the culture-contact situation which have given an impetus to the performance and dissemination of the ceremonies, and those which are already exercising an adverse influence. Unfortunately, she has been able to include only a small fraction of her field material in this volume, and it is to be hoped that she will publish more later; for, as Professor Lévi-Strauss points out in his preface, 'en montrant comment un culte religieux se transforme pour donner naissance à des pratiques qui relèvent de la magie, Mrs. Berndt apporte une contribution d'importance à la sociologie religieuse' (p. 5).

PHYLLIS M. KABERRY

**Economic Structure and the Ceremonial Exchange Cycle in Arnhem Land.** By Donald Thomson. Melbourne and London (Macmillan), 1949. Pp. 97, 11 plates, 2 maps. Price 12s. 6d.

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The subject of economics has been somewhat neglected by fieldworkers in Australia, with the notable exception of Dr. W.



Stanner who, in 1933-34, published in *Oceania* a report on the ceremonial economics of the Mulluk Mulluk and the Madngella Tribes of the Daly River. Dr. Thomson's small monograph is therefore welcome as an attempt to analyse the economic organization of Eastern Arnhem Land, an area for which we already have a considerable amount of material in the writings of Warner, Ronald and Catherine Berndt, and Elkin. He describes in some detail the ecology, technology, division of labour, and methods of collecting and preparing food; but he is mainly concerned with the ceremonial exchange of valuables, *gerri*, and the sanctions which maintain the system. *Gerri* are divided into two categories, the sacred and the profane, and include a wide range of weapons, implements, utensils and ornaments. Goods may change hands in barter or sale, but more frequently as free gifts, as goodwill presents for which there is no immediate obligation to reciprocate, as gifts for which an immediate return is expected, and as cumulative gifts to certain relatives, such as mothers-in-law. In all but the first two, the social or ceremonial rather than the economic aspect is stressed.

Dr. Thomson includes an account of the ownership of certain natural resources such as flint and ochre deposits, and the way in which artifacts manufactured in one district are distributed along ceremonial exchange routes in Arnhem Land and in the areas to the west and south. Individuals who engage in these transactions stand, as a rule, in certain degrees of kinship, e.g. mother's father—daughter's son, mother's brother—sister's son, mother's mother's brother—sister's daughter's son. A man may also exchange with his elder brother or his father's younger brother, but Dr. Thompson does not discuss the extent to which patri-clan ties are of importance as a basis for partnership. In fact, one of the weaknesses of this book is the lack of illustrative case material, though it does include a number of illuminating statements made by Aborigines with reference to their attitudes to partners and the sanctions behind their conduct. The exchange relationship is regarded as a life-long bond, and each partner strives to outdo the other on pain of losing prestige, or fear of sorcery, or of being passed over in a subsequent transaction.

Dr. Thomson's book is a contribution to our knowledge of the ceremonial exchanges of Arnhem Land, but it is unfortunate that he has not linked his findings with data on similar exchanges elsewhere in Australia. In footnote 3 on pp. 1 and 2, he refers to the system described by Dr. Stanner, and suggests that such cycles may have been widespread in Australia and overlooked by fieldworkers. But most unaccountably he himself appears to have overlooked a series of articles in *Oceania*, Vols. IX and X (1939), in which Mr. F. McCarthy collated all the existing data on 'Trade' in aboriginal Australia and 'Trade' relationships with Torres Strait, New Guinea and Malaya. Mr. McCarthy described the forms of gift exchange, the motives, the centres of distribution for such goods as red ochre, *pijuri*, pearl, baler and nautilus shells, types of boomerang and so on. He also distinguished and mapped seven major 'trunk routes' or 'trade routes': East Coast, South-Eastern, Cape York—South Australia, Central, Kimberley—Eyre's Peninsula, Kimberley—South-Western Australian, and North-West Australia Coast routes (*Oceania*, Vol. X, No. 1). During my own research in the Kimberleys of West Australia I found a ceremonial exchange similar to the *Merbok*, and have discussed it in some detail in *Aboriginal Woman* (pp. 166-74).

However, as far as Dr. Thomson's own material is concerned, one is disappointed to find little quantitative data. We are given an excellent account of activities during the year, but no demographic data on the size of groups inhabiting horde territories, on fluctuations in numbers from one season to another, and on the extent to which there is a concentration around mission and other European settlements on the coast. Again, while the motives for exchange are discussed in some detail, we are not told how many individuals within a horde enter into these exchange relationships, or what quantity of valuables passes through the hands of the individual during a year or a set period. It should be pointed out that in failing to provide us with such material Dr. Thomson does not differ from his predecessors, myself included, in the Australian field. One can only regret that he did not make a more complete break with precedent.

PHYLLIS M. KABERRY

**Stone-Age Craftsmen.** By S. R. Mitchell. Melbourne (Tait), 1949.

Pp. 211, 16 plates, 45 figs. Price 27s. 6d.

**276** The author of this very useful book is a geologist by profession, and it is the geological approach which gives the book much of its value. The first part consists of a general survey of Australian stone industries. It includes chapters on the types of stone used for artifacts; a classification of stone tools, based on function rather than on form; a detailed account of the various classes and sub-classes, their distribution, and the materials preferred for each; and a discussion of patination, showing the rapidity with which deep patination can take place when conditions are favourable. Mr. Mitchell emphasizes that the aborigine used any suitable flake found lying about, and often carried a piece of stone from which flakes were struck off as required; in either case the flake was frequently discarded after use. He quotes A. S. Kenyon (p. 29): 'there appears to be a tendency to consider as artifacts only those implements that show retouching, chipping, etc., whereas at least ninety per cent. of the primitive's use for cutting and scraping is of fortuitous flakes, so contemptuously disregarded or even thrown aside in Europe . . .'; and later comments (p. 34): 'Kenyon's statement that 90 per cent. of the implements used by primitive man for either cutting or scraping have not been retouched, may be an overstatement, but there is no doubt that large proportions used by the Australian stone-age people have not . . . This fact is highly significant, for in most classifications of the artifacts of an extinct stone-age people, only those that have been worked are included; the unworked flakes have been omitted, possibly because they were unrecognized or ignored as implements.' The author also stresses that the form of a tool as found is often the result of extensive retrimming, or simply of wear, and may not represent the original intention of the maker (p. 58); he quotes Aiston to the effect that 'most of the tools picked up by collectors are worn out and have been discarded.'

The second half of the book consists of an account of aboriginal camp sites, mostly in South-East Australia, which the author has investigated. It is not therefore exhaustive, but the specialist in Australian ethnography will find it very useful.

There is a very complete bibliography, and a good index, and the book is copiously and well illustrated. B. A. L. CRANSTONE

**De Bergpapoea's van Nieuw-Guinee en hun Woongebied.**

By C. C. F. M. Le Roux. Leiden (Brill), 1948 (Vol. I) and 1950 (Vols. II and III). 3 vols. Price 75 florins

**277** In two heavy volumes of nearly five hundred quarto pages each, illustrated with a large number of full-page photographs, Le Roux gives us what he calls the topographical and ethnographical results of the expedition which was sent out to New Guinea in 1939 by the Koninklijk Nederlands Aardrijkskundig Genootschap (Royal Dutch Geographical Society) and of which he was the leader. The third volume contains a large number of photographs, drawings and maps. The war made it difficult to finish the work, because of the severed communications with Indonesia. In September, 1947, Le Roux died. The text was ready, but much still had to be done to prepare the maps and photographs for the press. It was Mr. Visser, the author's son-in-law, who with the help of many others, was able to give the required finishing touch.

The title of the book promises more than we are offered. It does not treat of the Mountain Papuans of New Guinea in general, but only of those living in the western part of Dutch New Guinea. On the other hand it is much more than just a topographical and ethnographical account of the expedition of 1939. The author does not confine himself to enumerating the data which he and the other members of the expedition collected, but compares them with what others before him have collected and written about the same subjects.

Le Roux used to call himself an ethnographer, making a distinction between ethnology as an abstract science, and ethnography as a concrete science supplying the data to be used by ethnologists. This distinction will not be accepted any longer by the modern cultural or social anthropologist. Even a catalogue of an ethnographical museum presupposes a system by which the 'facts' are grouped, and this system can only be based on ethnological insight.



This is even more true in the case of a monograph. Le Roux's book shows this very clearly: it gives us more than the mere facts. When, for instance, the author mentions funeral rites or the use of tobacco or *sirih*, he adds many pages about the theories on these subjects. On the other hand the compilation and the explanations offered show a lack of theoretical schooling and professional knowledge, and as a result many pages give an accumulation of facts only, rather loosely held together by the headings of the various chapters and paragraphs. These facts may be useful in so far as they give information about cultures hitherto practically unknown. We should not, however, forget that Le Roux and his staff did not know the language of the people and could, therefore, at the best, enter into a superficial understanding with them.

Volume I contains chapters on a preliminary account of the first contacts with the Dutch; ornaments and mutilation; dress; manner of living, food, fire, salt, tobacco, *sirih*; means of subsistence; commerce and traffic; basket-work; tools; weapons and war. Volume II treats of: mentality; numbers, measures, calendar, points of the compass, gesture language; art; feasts; religion and magic; social and family life; moral ideas; betrothal and marriage; birth and the child.

H. TH. FISCHER

**Brown Men and Red Sand.** By C. P. Mountford. 2nd. ed. Melbourne (Robertson and Mullens), 1949. Pp. xv, 184, 63 plates. Price 17s. 6d. English edition: London (Phoenix House), 1951. Pp. 192, plates. Price 18s.

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Though this account of one of Mr. Mountford's expeditions to central Australia is designed for the general reader it has much to interest the ethnographer. His route lay among the Pitjendadjara people of the Musgrave and Mann Ranges, on the borders of

south, western and central Australia, and his main purpose was to record their mythology, which is nearly always associated with striking or important natural features—waterholes, hills or unusually shaped rocks. Many of these legends are recounted here, with photographs of the places associated with them.

Mr. Mountford emphasizes the richness in ceremonial and mythology of a tribe whose material culture is probably the poorest of any existing people, and the fact that this meagre material culture nevertheless represents an efficient adaptation to the environment. For the general reader this emphasis, and the space devoted to showing that the aborigine is friendly and reliable if well treated, are probably not yet superfluous. The book is very readable, and the photographs, as will be expected by anyone who has seen Mr. Mountford's films, are excellent.

B. A. L. CRANSTONE

**Südseestudien: Gedenkschrift zur Erinnerung an Felix Speiser.**

Basel (Museum für Völkerkunde), 1951. Pp. viii, 426,

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portrait, 26 plates, text figs.

This volume was intended to commemorate Dr. Speiser's seventieth birthday, but he died shortly before it, and his portrait shows him aged and tired. Among his many friends, seventeen have contributed essays in German, French or English, dealing with subjects related to his widespread activities: the editors are Hans-Georg Baudi, Roland Bay, and Hans Dietschy, and Professor Karl Meuli has written a sympathetic memoir. Of general interest are R. H. Lowie on *Some Problems of Geographical Distribution*, and Hans Dietschy *Verwandschaft und Freundschaft*, illustrated from Melanesian society, and M. Leenhardt *Le problème des migrations en Nouvelle-Calédonie*. The rest are descriptive and topical.

JOHN L. MYRES

## CORRESPONDENCE

**The Founders of Zimbabwe Civilization.** Cf. MAN, 1949, 80

280 Sir,—As Mr. G. A. Wainwright's article has proposed an entirely novel source for the derivation of some of our most striking monuments, I trust that you will be able to spare the space for a careful examination of his views, which I believe may be summarized as follows:

(1) On the basis of a passage from Ma'sudi (who visited the East Coast in A.D. 916-917) stating that the Zeng, a Galla people from Abyssinia, under the leadership of Waqlimi, had occupied the 'country of Sofala,' Wainwright suggests that these people gave rise to the Zimbabwe civilization. (2) This suggestion he fortifies by claiming (a) that the foundation of Zimbabwe has been shown by the beads recovered to have taken place in the ninth and tenth centuries A.D. and thus coincides with (b) the presence of phallic practices amongst the ancient peoples of Abyssinia and Southern Rhodesia, (c) the similarity of the pit dwellings of Inyanga to the towns of underground houses in Harar, and (d) the similarity of the 'Temple' ruin at Zimbabwe to the pre-Islamic temple at Marib.

(1) Ma'sudi's statement regarding the 'country of Sofala.' There seems to be a great deal of difference of opinion regarding the identity of the 'Sofala' Ma'sudi visited, for the word means no more than 'a promontory,' and Marconnes has, I think, shown conclusively that the Sofala of Gold, near Beira, was not occupied before A.D. 1150. Be that as it may, the term 'country of Sofala' could only apply to the coastal belt and not to high table land of the interior beyond the Escarpment Mountains, concerning which it is most improbable that Ma'sudi had any information. It is therefore evident that whatever country Ma'sudi had in mind in this passage, it was certainly not what is now Southern Rhodesia.

(2) (a) The date of the foundation of Zimbabwe. Wainwright bases his statement regarding the foundation of Zimbabwe on Beck's interpretation of the glass beads found there by Caton-Thompson and more particularly on those recovered from 'Test A.3' on the 'Acropolis' from which she derived her most decisive results. But even if we accept all Beck's conclusions, they do not show that the

place was founded during the ninth and tenth centuries, but merely that it was not of an earlier date.<sup>1</sup>

Caton-Thompson recognized one of the bedrock beads from this excavation as being identical with the Madi of the Venda ancestral beads. Indeed the relationship was closer than she thought, for, of the 80 glass beads recovered, 47 are either identical with or very similar to the Venda beads, and one bead, the 66 of her list, an Indian-red over clear green base cylinder, is generally of an eighteenth-century date,<sup>2</sup> or later.

If, however, these bedrock beads are to be used to set the dating of Zimbabwe to the ninth and tenth centuries, then we must give a similar date to the introduction of the Venda ancestral beads. But these beads are not guarded in treasuries, for many thousands of them are in daily use by the womenfolk of the chiefly families. It is altogether incredible that they could survive the vicissitudes of such use in such numbers for many generations.

Further, we know that before the Portuguese occupation, the 'Moors' were importing coloured clay beads from India,<sup>3</sup> and that the practice was continued under the occupation<sup>4</sup>; moreover, apparently similar beads have been found in India.<sup>5</sup> No actual evidence supporting Beck's identification of the bedrock beads from Zimbabwe and early glass beads from India has ever been brought forward, other than supposed resemblances, which, after comparing both sets of beads, I did not find very convincing. On the other hand, we do know that glass beads were brought to Sofala from Europe by the Portuguese, at the beginning of the sixteenth century.<sup>6</sup> These were the earliest glass beads of which we have any record in South Africa.

Caton-Thompson tells us that the rough stone structures which were built over and sealed the midden deposit in 'Test A.3' that yielded these bedrock beads were made after the West Wall of the 'Acropolis' had been erected.<sup>8</sup> While clearing the fallen debris of this wall in 1914, Douslin discovered the old entrance passage to the 'Acropolis' and on the floor of the passage remains of the wooden lintels that had carried the wall above.<sup>9</sup> Now there can be no doubt but that these lintels were contemporary with the work they supported, and there is no possibility that wooden lintels in such a position could last more than a century or so.



From all this I believe that the evidence of the beads tends to justify the dating of some of the more spectacular buildings at Zimbabwe to the eighteenth, rather than the ninth and tenth centuries. Nor is this entirely contrary to Beck's views, for when dealing with the beads from Matendere and Dhlo-Dhlo, all of which belong to the Venda canon, and can be dated with certainty from their association at the latter place with an eighteenth-century gin bottle, he stated: 'but as a whole, the beads are in rather better condition and are rather larger than most of those from bedrock sites, I could not say for certain that they are not of the earlier period.' And again, 'the yellows and the reds are indistinguishable from those of the early period.'<sup>10</sup> It is therefore difficult to understand why he should have suggested a chronological chasm of nine hundred years between the two sets of beads.

Neither is my proposal invalidated by Petrie's dating of some beads from Bulawayo to the fourth or ninth century.<sup>11</sup> These beads of red wound glass on a white translucent core, 0.30 inches long and 0.34 inches in diameter, belong to a group called *Chitalaka* in Northern Rhodesia and Nyasaland, where they were imported in large quantities by slave-trading Arabs. Livingstone ordered six *frasila* to be sent to him during his last journey. They were also current over a large part of South Africa during the latter part of last century.<sup>12</sup>

(2) (b) *Phallic practices in Southern Rhodesia*. I cannot comment on the phallic objects from Abyssinia mentioned by Wainwright, having had no experience in that field. Recent work in Southern Rhodesia has shown that the so-called 'phallic' objects from Zimbabwe, are really related to the *Mbusa*, as we term the stylized figurines used by many Bantu peoples in their initiatory ceremonies.<sup>13</sup> These are now usually made of wood or pottery,<sup>14</sup> but at Zimbabwe and Umtali<sup>15</sup> they were made of soapstone. It is, I believe, now generally recognized that it may be very misleading to classify objects, whereof the use is unknown to us, on typological grounds alone. Bent illustrated what he called an 'Ornate Phallus' and described it as having 'apparently a representation of a winged sun on its side or perchance an Egyptian vulture.' As a matter of fact it is a stylized female figurine, the 'winged sun' being a protuberant umbilicus, flanked by the tribal cicatrization marks, while the rosette at its apex, likened by him to a Phœnician sun symbol, really represents, I would suggest, the local style of doing the hair.<sup>16</sup>

(2) (c) *The pit dwellings of Inyanga*. I am again at a loss regarding the towns of underground dwellings of the Harar area, but it is very unlikely that they can have any relation to the Inyanga structures. These consist of a partly subterranean cattle pen, surrounded by an earth platform whereon four to six dwelling and store huts were erected.<sup>17</sup> These structures are scattered over the countryside, each having been the domicile of a little community, practising agriculture with the herding of diminutive cattle.

(2) (d) *The relation of the 'Temple' at Zimbabwe to that at Marib*. Underlying any attempt to correlate the 'Temple' at Zimbabwe with the pre-Islamic temple at Marib, or indeed any other building, there must be the idea that the former was built to a definite plan and that the form of the completed structure was visualized from the beginning, but in fact no such plan can ever have existed, for it is obvious that its present form is due to a series of adaptations, the latest being the construction of the great girdle wall at a date consistent with the survival of the wooden lintels carrying the masonry over the north-west entrance until 1891.<sup>18</sup> The work was abandoned before it was completed, and we shall probably be not far wrong if we attribute this to the Swazi invasion of about 1830, a date that agrees very well with the growth of the trees within the walls.<sup>19</sup> In these circumstances, I believe, any resemblances there may be to the temple at Marib, that was deserted in the early seventh century, must be entirely fortuitous. I would suggest that this wall was built in order to adapt a structure used for some such initiatory rites as the *Kgoma* of the Pedi to serve as a *Thondo* enclosure of the Venda pattern.

In conclusion, I do not wish to rebut Wainwright's main contention—that the Zimbabwe civilization has its roots in the north, but to demonstrate the inadequacy of the grounds upon which he bases it. I would like to suggest that more fruitful results would

flow from a comparative study of the royal institutions of the Monomotapa state and those of the Bahima-Batutsi, such as are typical of Uganda, Unyoro and Ruanda. J. F. SCHOFIELD, A.R.L.B.A.

#### Notes

- <sup>1</sup> F. Marconnes, 'The Two East African Sofalas and King Solomon's Ophir,' *NADA*, Vol. XIII (1936), pp. 39ff.
- <sup>2</sup> G. Caton-Thompson, *Zimbabwe Culture*, Oxford, 1931.
- <sup>3</sup> D. Randall-Maciver, *Medieval Rhodesia*, London, 1906, p. 82.
- <sup>4</sup> G. M. Theal, *Records of South-Eastern Africa*, London, 1898, Vol. VI, p. 277.
- <sup>5</sup> *Ibidem*, p. 368 and many other passages.
- <sup>6</sup> E. F. C. Murray, 'The Ancient Workers of Western Dhalbum,' *J. R. Asiat. Soc. Beng.*, Vol. VI (1940), No. 2, pp. 79-104.
- <sup>7</sup> Theal, *op. cit.*, Vol. II, pp. 26f.
- <sup>8</sup> Caton-Thompson, *op. cit.*, p. 80.
- <sup>9</sup> H. Douslin, 'Recent Explorations at Zimbabwe,' *Proc. Rhod. Sci. Ass.*, Vol. XX (1920-1).
- <sup>10</sup> Caton-Thompson, *op. cit.*, p. 238.
- <sup>11</sup> G. A. Wainwright, 'Early Foreign Trade in East Africa,' *MAN*, 1947.
- <sup>12</sup> J. F. Schofield, 'A Study of the Old Trade Beads of Nyasaland,' *Trans. Roy. Soc. S. Afr.*, Vol. XXX (1943), pp. 2-34.
- <sup>13</sup> Audrey I. Richards, 'Pottery Images or *Mbusa* used at the Chisungu Ceremony of the Bemba People of Northern Rhodesia,' *S. Afr. J. Sci.*, Vol. XLI (1944), pp. 444-58, illus.
- <sup>14</sup> J. F. Schofield, *Primitive Pottery*, Handbook No. 3 of the S. Afr. Archaeol. Soc., Cape Town (Rustica Press), 1948, Plate VI.
- <sup>15</sup> Maciver, *op. cit.*, Plates XIV and XV.
- <sup>16</sup> J. T. Bent, *Ruined Cities of Mashonaland*, 3rd. edn., London, 1902, p. 188.
- <sup>17</sup> J. F. Schofield, 'A Survey of the Recent Prehistory of Southern Rhodesia,' *S. Afr. J. Sci.*, Vol. XXXVIII (1942), pp. 81-111, illus.
- <sup>18</sup> Bent, *op. cit.*, p. 109.
- <sup>19</sup> J. F. Schofield, 'A Critical Examination of the Building Methods Employed,' *S. Afr. J. Sci.*, Vol. XXIII (1926), pp. 971-86, especially 985.

#### Bush Negro Calabash-carving. Cf. MAN, 1951, 97

SIA.—It is regrettable that the analysis of the formal aspects of Bush Negro calabash-carving by Philip Dark should be marred by questionable ethnography and pseudo-psychological conjecture.

Thus, for example, one wonders at the source of his statement—he gives no references—wherein he speaks of 'a certain lack of permanence' in 'marital relationships,' in which 'the women appear to have the whip hand' and to be 'able to break a union on slight pretext.' This would, on the face of it, seem to be especially difficult to envisage in a culture which, in Mr. Dark's own terms, is 'extremely well integrated.' Mrs. Herskovits and I, during our field trips among this people, found no evidence of this sort of social chaos.

One cannot but ask, moreover, why Mr. Dark singles out for special comment the fact that the creative artistry of women in the medium he discusses is so different from that of men, and finds it so significant that two different art styles, based on sex division of labour, exist here and in other cultures. That the sexes differ in their patterned interests, and that these differing interests express themselves creatively in different ways, would seem to follow logically, as it does ethnologically, from the fact that the line of sex differentiation runs through all aspects of culture.

Certainly the suggestion that the differences between the characteristic styles of calabash-carving of Bush Negro men and women reflect 'a distinct difference in temperament between the men and women of this culture' is most unfortunate. There is no evidence of such difference known to me, either in the literature or in my own field experience, any more than there is to support the assertion that 'the men would appear to be definite in their actions and probably more conservative than the women,' while 'women may be thought more fickle and more easily susceptible to novelty.' As a matter of fact, the men are the innovators in this culture, the women less susceptible to novelty. Before 'specific correlations' are made between the variety of forms in Bush Negro art and 'the tenuous



quality of the marital relationships,' one might ask at least that the validity of the variables to be correlated be established.

Mr. Dark has made a contribution to the comparative study of art by his analysis of the formal aspects of Bush Negro calabash-carving. The more difficult problem, whereby drives toward creativity are analysed must, however, be approached quite differently than through the kind of speculations in which he indulges, if we are to understand the historical and psychological causes of the observed varieties of expression with which he is concerned.

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### Epe-Ekpe: The New Year Festival of the Glidji Ewe

**282** SIR.—The following account supplements the writings of Westernmann (*Die Glidji-Ewe*, Berlin, 1935) and Spieth (*Die Ewe-Stämme*, Berlin, 1906; *Die Religion der Ewe in Süd-Togo*, Göttingen, 1918; *Unter den Ewe in Togo*, Bremen, 1921). I am indebted to the Wenner-Gren Foundation for an award which enabled me to undertake an ethnological study of the Ewe in 1948.

The New Year of the Glidji Ewe falls at the end of the third week of September. At the end of the maize harvest, when the year's cycle draws to an end, the priests of Sakuma share stones among themselves to determine the time cycle. They begin to reckon the time to the New Year and they give orders forthwith that there should be no further beating of drums or loud singing, and that the children should be quiet until the priests raise the ban at the beginning of the New Year. From now on they watch the sky and are on the lookout for the new moon; as soon as they see it, they inform the people that the New Year's moon has appeared. The people brew *liha* or malted corn beer, and the old men watch the sea until a fine shower falls and a whale throws a fine spray; then is the New Year. All entrances to the groves of deities are now decked with new cloth and fresh palm fronds.

The ancestors of the Ewe of Glidji were originally of the Gã tribe (called Gẽ or Mina in Eweland) of Accra on the Gold Coast and they had the habit of celebrating their traditional feast in honour of their king. Their founder was Foli Bebe, who came to the country from Accra in 1680, bringing with him the Gã royal stool (actually an ivory head rest) covered with gold leaf, which he hid in hide and cloth at Zowla a few miles from Glidji, where it still is. The Gã demanded this stool back and waged an unsuccessful war for its recapture. Their descendants in Eweland celebrate this traditional feast called *Epe-Ekpe*, extending over eight days, at the end of the thirteenth lunar month each year; among the Ga it is called *Honowo*. The king makes an offering to the stool at the New Year, before he goes to war, and whenever he dreams of the stool.

I was fortunate enough to be invited to attend this celebration in 1948 both at Glidji and (a week later) at Anecho. I shall describe the feast of Glidji as the more important historically, and because the king of Glidji ranks higher.

Thursday, 23 September, was *notagbe* or *kperagbe*, the 'day of cleansing' or 'day of lifting of stone.' In the early morning the whole population took part in sweeping and cleaning the main street and all the streets along which all the Gã people from far and near were to pass in the afternoon on their way to invoke Sakuma, the god of war and protector of warriors, and to lift the sacred stone, painted white and red, in front of the temple of Kõle the goddess of fertility, at Glidji-Kpodji. This stone is believed to possess the power of foretelling whether it would be a good or bad year according to the side it presents to Sakuma—red being bad and white good. Priests, known as *wolumo*, from Tugban, Elias, Nugas and Gbugblan who are followers of Sakuma, Kpessu (god of war and blacksmiths), Kõle, Lakpan or Ekpan, Nyigbleon (god of comets), Djobu or Gbodo (god of yaws or frambœsia), etc., preside over this ceremony.

Friday, 24 September, was *nualigbe* (or *wliigbe*), the day of remembrance of the dead. From early dawn there were visits to the graveyards and general preparations were being made. One saw relatives tidying and decorating the tombs and placing food offerings on them. Everything was swept and the place made tidy. In the evening there was a general wake and at nine in the evening the drumming of the great drum or *atopani* began at the royal court at

Ahuegame in Glidji, giving the signal to all the neighbouring villages and townships to begin drumming (strictly prohibited for the month and a half before *Epe-Ekpe*). The wake was accompanied by much drum-beating, singing and libation-pouring in honour of members lost during the year, and lasted till midnight.

Saturday, 25 September, was *nuhlegbe* or *yekẽ-yekẽ-dugbe*, the day of public banquet. On this day *yekẽ-yekẽ* (ground miller and ocroo mixture) was sprinkled on the tombs. At dawn drums sounded from all directions; all families gathered together in their homes, pouring libations, and rams, goats, chickens and even oxen were sacrificed before the royal stools in special honour of ancestors. Towards midday, specially prepared food was sprinkled over the ancestral tombs and stools and everybody was allowed to sit down to a free meal at the banquet, whence the name *Yaka wo kae* ('It was spread free of charge') given to this principal festive meal.

Sunday, 26 September, was *nlowanagbe*, New Year's Day, the day of greetings and mutual congratulations. On this day people greet each other in the Gã or Accra dialect with *gwla, gwla* ('long life, long life!'), and quarrelsome neighbours are reconciled. From early dawn women make visits to those homes where parents have died recently or in the course of the year to weep and wail for the departed. Tradition obliges them to recognize by this act the greatness of divine providence which has preserved them to witness the beginning of the New Year. The drumming, songs and festivities, forming a complex of rhythmic movements and rejoicing, emphasize the ties uniting the quick and the dead. All day greetings are exchanged thus:

Gã: *Dwula! gwla! gwla! ewla, muola! afi naakpel akpe boboo! . . .*

Ewe: *Evõe ne to dji enyue neva anyigba dji! teti nenw agbe ne tekũ ne vã do enti! . . .*

Long life! Re-awakening in all things!

Good fortune without a cloud!

May evil pass above and good fortune descend upon the earth.

May the elders live and remain as support to uphold the young . . .

New Year's Day is especially the king's day for receiving men and women of distinction and giving the customary banquet, after which everyone joins in the universal merry-making. The populace organized dances and songs to fete the king in the royal palace or in the main square at Huntitogome at Glidji. He distributed silver and drink to his chiefs and chief lieutenants, ministers, warriors, notables and royal aunts, wishing them *gwla, gwla*, for good services rendered to their native land.

Monday, 27 September, was *gwlanagbe-be-djola*, or 'Festal Monday.' Parades and visits took place, and those who had been prevented by the celebrations on Sunday from paying calls on their friends were free to carry out their pleasant tasks. Boys and girls forming separate groups sang folksongs in the streets, wishing *gwla* to the people in the homes where they gathered various presents.

Tuesday, 28 September, *vodu-Do-Hegbe* or *kpa-to-gbe*, is the ritual closing of the festivities and is marked by the re-entry of Sakuma to Glidji-Kpodji. In the afternoon there is ritual drumming at Gbatsunu. As at the opening on Thursday, the principal Gã of Glidji, Anecho, Porto Seguro and Agoo sent delegations to attend the closing of the festivities at Glidji-Kpodji. On this day there took place, in the Ella district of Anecho, the coming-out of the deity Lakpan.

On Wednesday, 29 September, *kpatagbe*, there was a religious dance in the Ella district, where all the adherents of Lakpan collected to carry out their annual rites.

Thursday, 30 September, was *kpayi-ho-gbe*, the day of the entry of Lakpan which closed the Holy Week. From the religious point of view the ceremonies and festivities were over; but various rejoicings and celebrations followed their course and the different families continued their own celebrations as well.

Strong delegations are sent to the festival from the original home of the people at Accra, whose priests keep in touch with their descendants and instruct them in the sacred lore.

Londonerry

R. E. G. ARMATTOE









(a) The 'Accident Scene'



(b) Both sides of a broken dart-thrower in the form of a bird with smaller birds forming its wings



(c) Bison at Altamira bearing signs similar to those on the Lascaux bison



(d) Bison at Altamira bearing signs

(a) After Windels; (b) from IPEK; (c, d, e) after Stiegelmann



(e) Similar signs placed in front of an animal (right)



# THE INTERPRETATION OF THE 'ACCIDENT SCENE' AT LASCAUX\*

by

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**283** Among the many groups of animals painted on the cave walls at Lascaux<sup>1</sup> there is one in particular which has attracted the attention of scientists and challenged interpretation. It is located at the very bottom of a shaft (fig. 1), and is composed of a bison and a man in a bird mask lying prostrate in front of the animal (Plate La). Nearly all scientists consider this the unit of composition, but the woolly rhinoceros at the left of the hunter



FIG. 1. THE BOTTOM OF THE SHAFT IN THE CAVE AT LASCAUX

Part of the 'accident scene' is seen at left. After Windels

could also be a part of it—particularly as it is pictured in a violent charging position, as though it had trampled the man, whereas the bison stands and turns its head backward, and is therefore not in action.

In the film *Lascaux: Cradle of Man's Art* made by William Chapman in 1950 (Gotham Films, 31 East 21st Street, New York), he says of this scene: 'In a deep and inaccessible shaft off by itself they (the boys who discovered the cave) found this group—a man apparently dead and with a bird's head—nearby a totem-like bird on a stick—a woolly rhinoceros—a bison pierced through with the spear—his

\*With Plate L and 3 text figures

entrails coming out—of what ancient tragedy does this tell?' In a letter to me Mr. Chapman says: 'I was not prepared to stick my neck out to the extent of saying that this is the earliest example of man and his totem known, although I believe this to be so. For the direct reference to "entrails coming out," I am indebted to the Abbé Breuil, who felt the man had thrust his spear through the bison just before he was killed by the woolly rhino, who is walking off stage left.' It seems apparent to me that the rhinoceros, and not the bison, killed the hunter.

Now let us discuss the other details of the picture. There is a dart on the ground of the type usually thrown with a dart-thrower. Its tip is typical of the late Aurignacian, Solutrian and very early Magdalenian (fig. 2, after Obermaier, *Fossil Man in Spain*, New York, 1936, p. 209). Such tips were made of hartshorn, ivory or bone; they were cut on the slant at the base and attached to the shaft, so that the dart had not only a sharp point, but also a single barb. The dart gives meaning to the bird on the stick: this



FIG. 2. UPPER PALÆOLITHIC DART TIPS

(a) Solutrian; (b) Magdalenian; (c) supposed manner of use. After Obermaier

is the dart-thrower or *atlatl*, or as the French would say, *propulseur*. At its lower end is a short protrusion which was obviously designed to facilitate holding, and to ensure a firm grip and thus prevent the dart-thrower slipping from the hand when swung in full force by the hunter. The artist was forced to picture the dart-thrower vertically in order to show the important detail of the bird, since palæolithic artists had not mastered the technique of perspective. The strict side view was necessary for clarity; even the Egyptians 25,000 years later used the same method.

The idea of a bird totem, fascinating as it may be, could be discarded if it could be proved that dart-throwers existed



at that time with such birds carved at the end, the bill forming a hook to hold the dart when being thrown. Fig. 3 shows a completely preserved dart-thrower excavated at the Larthet cave, Ariège, France; the end is carved in the form of a monkey head, and the handle is worked to fit the hand. Fortunately, a broken dart-thrower has also been found which has a bird at its end, as Plate Lb shows.<sup>2</sup> It is particularly interesting as a work of art because the wings of the bird are formed by smaller birds carved in relief, and not identical with each other. The bird is, by its magic,



FIG. 3. DART-THROWER WITH MONKEY HEAD (AT LEFT)  
After Obermaier

quite appropriate to the purpose, since its *mana* would endow the dart with 'wings' and with the flying power of a bird.<sup>3</sup>

The manner in which the long spear is drawn does not indicate the piercing of the bison; it may have been lying on the ground like the dart, or stuck in the ground at an angle. This conclusion may be drawn from the way its shaft runs in front of the animal up below the bison's tail; it is clearly pictured as in front of it. We have numerous palaeolithic figures of animals which have been struck by weapons and this fact is always indicated very directly and in the direction of the body; in Windels's *Lascaux* itself over a dozen such cases may be found.

Under the body of the bison is a sign shaped somewhat like the Hebrew letter *sin* or *shin*. The Abbé Breuil interprets this design as representing the entrails, as noted in Mr. Chapman's letter quoted above. It is reasonable to believe that this interpretation is also inaccurate. This sign, which may have three, four or five parallel lines, occurs sometimes above the animal, sometimes on the body but not in the intestinal area. The example shown in Plate Lc is chosen because it is in this case clear that the signs were placed on the animal because their symbolic character cast a kind of spell on it. To substantiate the existence of such 'formulae' added to animals, Plate Ld is shown in which these signs appear like inscriptions on the bodies of the bison. In Plate Le the same 'formula' is seen as on the upper bison of Plate Ld, but here it is placed underneath or in front of an animal; a variation of a 'trap sign' is visible above the bison to the left. I would like to call the sign on our Lascaux bison a 'trap sign.' We should accept the interpretation of Piette, who, after the discovery of the pebbles with painted signs at Mas d'Azil, insisted that all these palaeolithic, mesolithic and neolithic signs are symbols or magic ideograms used for spells, and that they were the forerunners of writing (*L'Art pendant l'Age du Renne*, Paris, 1907).

If the bison is not wounded, what is then the meaning of the whole composition? Only Max Raphael has so far tried to analyse and study palaeolithic art from the view-

point of aesthetics, philosophy and sociology combined for the purpose of bringing into the discussion general metaphysical aspects (*Prehistoric Cave Paintings*, Washington, D. C., 1945). He showed that palaeolithic art is not primitive at all, not a beginning—art in its birth—but very complex; and that not only magic of the hunt and fertility, but also the social struggle of totemistic clans was involved. Admittedly, he was working with hypothetical assumptions; but the totemistic interpretation of the large compositions at Altamira, Font de Gaume and Combarres seems very likely to be correct. He concludes that the horse and the bison represent the corresponding human clans, a more peaceful human group 'trapped' in their struggle for life, while the bird-masked man represents another aggressive type of bird clan, overcome by the life struggle and wiped out by brutal nature or by another clan, represented by the rampant rhinoceros.

I hardly dare to propose such an interpretation of this scene, but it may serve as an example to illustrate how complex and symbolic the whole group could be if we accept Raphael's line of argument. We should have courage enough to admit that only to a very small extent are we able to interpret palaeolithic art.

We may conclude, however, that the bird on the stick is not a symbol, but a dart-thrower and part of the hunting equipment of the prostrate hunter.<sup>5</sup>

#### Notes

<sup>1</sup> See for example H. Breuil, 'Una Altamira Francesa, La Caverna de Lascaux en Montignac,' *Archivo Español de Arqueología*, Madrid, Vol. I, 1941, pp. 361-86; Fernand Windels, *Lascaux, 'Chapelle Sixtine' de la Préhistoire*, Montignac, 1948.

<sup>2</sup> Found in the entrance to the cave of the Trois Frères near Montesquieu-Avantès, Ariège, France; see H. Kuhn, 'Die Höhlen Trois Frères und Tuc d'Audoubert,' *IPEK*, Berlin, 1930, Vol. VI, pp. 115ff. and Plate 3.

<sup>3</sup> Josef Roder has taken issue with the interpretation of the stick with bird as a totem pole (*Pfahl und Menhir*, Neuwied, Germany, 1949, p. 10). In view of the findings of H. Rust (*Nachrichtenblatt für Deutsche Vorzeit*, Vol. XIV, 1938) at Hof Stellmoor near Ahrensburg, he interprets it as a sacrificial pole or something linked with it. Rust had found a pine pole two metres long pointed at both ends with a reindeer skull still fixed on one end; originally this pole had stood upright at the edge of a small pond in which the submerged carcass of a reindeer was found. The reindeer had been used as a sacrifice, for a stone weighing eight pounds had been placed inside its chest. (The site is dated by Pollendiagram to 7000 B.C.)

Roder also points to the sacrificial scene on the sarcophagus of Minoan origin found at Hagia Triada, Crete (Evans, *Palace of Minos*, 1921, Vol. I, p. 440). He considers the 'obelisks' appearing on it to be made of wood rather than stone, since the garlands were wound around them. On the top are double axes in the shape of a cross, indicating the four directions of the compass; a bird may be seen perching on each of them. The line of argument is suggestive, but I do not propose to apply it here, since the Lascaux piece is a dart-thrower and fits perfectly into this hunting scene.

<sup>4</sup> Plate Lc-e are after A. Stiegelmann, *Altamira, ein Kunsttempel des Urmenschen*, Bonn, 1910.

<sup>5</sup> Mention should perhaps be made, however, of Mr. F. de F. Daniel's suggestion (*MAN*, 1950, 65) that the bird on the stick may be compared with the *burtu* technique practised by hunters in the Western Sudan, in which a realistic representation of the head and neck of a ground hornbill is mounted on the hunter's head; he then imitates the bird's movements as a cover, while stalking his prey through the long grass. This suggestion would tend to support another interpretation of the 'accident scene,' under which the



'barbed dart' lying to the right of the bird is itself regarded as a dart-thrower or spear-thrower, while the bird is rather a decoy; in that case the otherwise unexplained crosspiece near the butt end of the 'dart' would provide the grip of the thrower, as in many such implements among modern primitives. It might also follow that the artist did not intend to represent the supine figure as wearing a mask; the schematization seems in any case somewhat careless. But

no doubt other interpretations of the scene are equally possible, and the Hon. Editor would welcome discussion of the matter in MAN; it is one in which, by skilful—or lucky—research, 'reconstructed history,' even of 30,000 years ago, might be taken beyond the bounds of mere conjecture. Perhaps the representations used in the transmission of tribal lore to initiates among many African and other peoples may not be irrelevant to the discussion.—Ed.

## MISCELLANEOUS NOTES ON THE KUKI OF THE CHITTAGONG HILL TRACTS, PAKISTAN

by

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**284** The following observations were made in the Kuki village of Dralaukhanpara and the hamlet of Saibulsipara, about five miles distant from it. It is one of the 16 Kuki villages located south of the left bank of the Karnafully river. The main purpose of my visit to the Chittagong Hill tribes, which took place in early September, 1950, was to make a preliminary exploration of fieldwork facilities among the Mog.<sup>1</sup> As opportunity offered I tried to gather wider information on the kinship systems prevailing in the area.<sup>2</sup> The remaining observations (which are highly fragmentary since the information was not collected purposefully, but was obtained incidentally during a rapid survey made with a wholly different outlook) have been gathered in the present paper. It will be sufficient to recall that the Chittagong Kuki number 5,000 and are closely related, both linguistically and culturally, to the neighbouring Lushai farther north and east, though they seem to avoid intermarriage with them. The village where I spent a few nights had about 60 inhabitants and was situated on a hilltop (access to which was made extremely difficult by the rains, which were at their peak) about 10 miles south-east of Subilong, a small bazaar on the left bank of the Karnafully river, five to seven miles upstream from Rangamati.

### *A Kuki House*

The house in which I lived was rectangular, about 18 metres long by 7 metres wide. It was built on a slope, so that while the front part was elevated on piles about 1 metre from the ground the height of the piles in the rear was not far from 3 metres. The walls and the floor were made of split and flattened bamboo poles plaited in checkwork. The Kuki names are as follows: house, *in*; post, *ind'tung*; floor, *suang*; ceiling, *sungta*; roof, *pal*. The house consisted of four main parts:

(1) The veranda (*sampek*). This was open on the front and both sides and about 2 metres deep. Access was by a ladder (*lailak*) made from a notched tree trunk. When one faced the house, the right corner of the veranda was (as in all the other houses) occupied by a huge pile of firewood reaching up to the

roof and so covered with dust and spiders' webs that it had obviously never been drawn upon. We were told that this display was done mostly for prestige, in order to show that the womenfolk of the house were active and industrious. Thus, the woodpile was the females' counterpart of the hundreds of skulls (monkey, deer, boar, panther, hornbill, etc.) which completely covered the back wall of the veranda, the small skulls impaled in groups on long sticks, the larger ones set in individual frames of basketry.<sup>3</sup> By the woodpile was the mortar (*rasum*), a big wooden block about 1 metre wide with a small cavity in the centre, and a pair of pestles (*sandre*), almost as high as a man, which are always used alternately by two girls pounding paddy in the same mortar. On the left side of the veranda the poultry were kept, either free or in numerous cage baskets. The pig trough (*yok sak-rakung*) was situated outside near the ladder. A water jar by the door of the main room was intended for washing the feet, a constant precaution of the Kuki before walking on the beautifully polished bamboo floor inside.

(2) The main room (*insung*). This was about 11 metres long, divided by a double row of pillars (11 on the left and five on the right) into three aisles: the left aisle intended for guests and the mens' gatherings, and implements (for instance peller bows, *sairu*, were hanging on that side), a central alley, and the right aisle. The latter was occupied first by the clay hearth (*darsap*), surrounded by a square framework of bamboo for storing the kitchen utensils; then a free space (about 7 × 2.5 metres), the place for the women and their work, spinning and weaving apparatus being kept by the back wall;<sup>4</sup> and then two sleeping rooms (*inkhumg*) or rather cubicles raised on higher posts about 30 centimetres above the floor level and surrounded with matting screens. The first and larger one (about 3 × 2.5 metres) was occupied by the man of the house, his wife and his two unmarried daughters; the second (2 × 2.5 metres) by his son and his wife and baby. In the recess between the last cubicle and the back wall was set up a display of five or six superposed buffalo skulls (*solu kaetar*) from the sacrifices. These were commemorated outside the houses by a few forked posts (*songg*) with button-shaped ends and carved wider at the fork. However, all the 25 buffaloes owned by the villagers had died a few years before from rinderpest, and the pitiful economic situation of the natives, their *shums* invaded by *sumn*-grass, the paddy crop below subsistence level and their orange trees dying out, did not permit them to be replaced.

(3) The paddy house or granary (*s'et*). This square room opened on the left aisle and was an adjacent building. It was almost entirely occupied by three cylindrical basketry grain bins (*pang*), the larger one about 1.5 metres wide and 2 metres







obliquely in relation to the median plane of the body. As in the first step there was a bending of the knees each time the soles were replaced on the ground.

The natives call the first step (heel and sole raised) *anso*, and the second (toes and sole stamped) *ansai*. The low tone of the drum is called *pu-i*, and the high one *sal*. However, it seems that these metaphorical equivalences are somewhat inverted for the Kuki and that our 'dark' tone may be the stronger one and our 'clear' tone the weaker.

The girls were wearing a short skirt reaching to the knees, a breastcloth and a kind of bolero; they were heavily loaded with several rows of copper tubes (*ruiseng*) round the hips, silver jewellery of local origin, amber necklaces (*sana*) traded on the Burmese border and necklaces made of discs carved from the shells of river snails (*sanko*). They also wore large earplugs (*labe*) made of horn, bone or ivory, and the borders of their breastcloths were ornamented with the green-gold wings of some kind of beetle (*i'arsumser*).

Older women were squatting and watching the show, smoking their waterpipes (*dumbel*), one of them holding her baby who was showing a typical Mongoloid spot (Kuki: *abom*; Šakma: *kalodak*).

#### Kuki Games

Once, about 4 p.m., the young people started playing games. Each boy seems to own a top of his own fabrication. This is made of a large, flat, round seed of a creeper (*Entada pursoetha*) pierced by a thin pointed stick, and resembles a primitive spindle. To make the top spin, a piece of string is rolled round the stem and the end of it is drawn through a square hole cut in the middle of the wider part of a crude triangular bamboo lath. This piece is held in the left hand, the first finger holding the top against it in a crosswise direction, the whorl upward. Then, by a sudden drawing of the string through the hole with the right hand while pushing the top forward with the left hand, the top is set in motion; in fact, it jumps down to the ground where it starts spinning with a fierce hum. The game called *lamsir* consists of trying, as soon as the first player releases his top, to 'kill' it with the remaining tops, quickly but carefully aimed at it by the other players.

Another game, called *konyon* by the Šakma, *poienka* by the Kuki, has often been described; it seems to be widespread in south-eastern Asia.<sup>7</sup> However, the Kuki have elaborate ways of playing it. At first, boys and girls play separately. Each player in turn sets up on the ground a row of four to six seeds of the kind described above, and the

other players try to knock down as many as possible by throwing another seed at them. The interesting point is that the ways of throwing the 'ball' seem to vary indefinitely and are not the same for boys as for girls. With the boys, the first method consisted in holding the 'ball' in the right hand and the right foot in the left hand; then, after hopping five or six times on the left foot to gain impetus, the player threw his projectile from under the bent right knee. After all the players had performed that way, a new method was introduced which consisted in holding the ball between the ankles and trying to throw it with a forward jump after a few preliminary leaps; a third method was about the same except that the seed was held against the inner part of the right leg by the sole of the left foot. Then, returning to a simpler method, the players decided to throw the seed by hand, rolling it along the ground, etc.

The girls' methods can be quite as sophisticated: they hop on the left foot, the right foot raised above the ground with the seed lying flat on the instep; or they put it in their mouth and after taking a short run they try to spit it at their aim; or they hop on one foot with the seed on the top of their head and try to throw it with a sharp nod. Many other methods were noticed: the seed thrown backward and between the legs, or backward and over the head, or forward between the legs, etc. After the game has been going on for some time, the girls' team and the boys' team become mutually excited with their shouts and laughter; then they join and try each other's techniques; these often unsuccessful attempts add to the general merriment.

#### Notes

<sup>1</sup> See my forthcoming article 'Le Syncretisme religieux d'un village mog du Territoire de Chittagong.'

<sup>2</sup> These will be dealt with in another paper, 'Kinship Systems of three Chittagong Hill Tribes,' to be published elsewhere.

<sup>3</sup> Exactly similar to those published 65 years ago by E. Riebeck, *The Chittagong Hill Tribes, Results of a Journey made in the Year 1882*, London (1885), Plate 8.

<sup>4</sup> These women's utensils included: giining press (*namot*), carding bow (*laka*), spinning wheel, with horizontal polygonal spindle (*mutir*), and distaff (*loson*), yarn (*lomui*), some of it black (*mlahum*), raw cotton (*malekung*), and weaving looms (terms not recorded).

<sup>5</sup> The Kuki cultivate wet and dry rice, sesame, mustard, tea, bananas, oranges and maize. The last, called *minsai*, produces small cobs set with large irregularly polygonal kernels. It includes several varieties: white (*anomi*); purple (*polmi*); variegated (*rielmi*).

<sup>6</sup> Cf. Riebeck, *op. cit.*, Plate 15, figs. 5, 7.

<sup>7</sup> Cf. Shway Yoe, *The Burman*, 2 vols. (1896), p. 373f.; E. Porée Maspero, 'Cérémonies des douze mois; fêtes annuelles cambodgiennes,' *Commission des Mœurs et Coutumes du Cambodge* (1950), p. 24.

## SHORTER NOTES

**The First International Congress of Peruvianists, 1951.** A report by Dr. G. H. S. Bushnell, Curator, Cambridge University Museum of Archaeology and Ethnology

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A series of congresses have been held this year in Lima in connexion with the fourth centenary of the foundation of the University of San Marcos. The First International Congress of Peruvianists, which took place in the latter half of August, was

the latest of these, and its success amply satisfied the aims of its promoters. One of the resolutions passed at the end of the Congress was that similar ones be held at five-yearly intervals. It is much to be hoped that this will be possible, since it should do much to stimulate research, particularly on Colonial history. The Organizing Committee invited a number of foreign delegates, of whom I had the honour to be one, and, with great generosity,



paid their travelling and other expenses. Everything was done to make the delegates welcome in Peru, and they were received by the President and the Minister of Education as well as the Rector of the University. In addition I received every assistance from the local representative of the British Council, through whom the invitation was originally extended.

The subject of the Congress was essentially historical, but it included an archaeological section under the heading of Pre-Spanish Peru. Concurrently there was an anthropological congress, nominally separate, but actually most of its sessions were arranged so as not to clash with those of the Peruvianists. This was concerned with physical anthropology, particularly with medical matters such as the effects of altitude on the human body and the relation of the incidence of the diseases of the Andean region to topography, both of them burning questions in Peru. Dr. Carlos Monge, Director of the Institute of Andean Biology in the University, and Dr. T. D. Stewart of the Smithsonian were prominent in this Congress. Among the functions connected with it was an interesting demonstration by Dr. P. Weiss of the evidence of disease and of surgery, particularly trephining, in the rich collection of pre-Columbian human skeletons in the National Museum.

The President of the Peruvianist Congress was Dr. Raul Porras Barrenechea and the secretary Dr. Luis J. Cisneros, both of the University of San Marcos, and a prominent part was taken by Dr. Aurelio Miró Quesada Sosa, Dean of the Faculty of Letters in the University. Many other Peruvians, whose names I have not space to record, took an active part, but I cannot forbear to mention Doctor Ella Dunbar Temple, to whose initiative the organization of the Congress was largely due, and Dr. Luis A. Valcárcel, the distinguished archaeologist, both of whom we had the pleasure of welcoming to this country last year. Among the foreign delegates, I can only mention a few who may be known to archaeologists and anthropologists. Dr. Rivet was there, as active as ever, and so was Dr. Henri Lehmann of the Musée de l'Homme. The French contingent also included Dr. Henri Reichlen, who was working in Peru at the time. Dr. Hermann Trimbom came from Germany. Among a number of Spaniards were Dr. Ballesteros of Valencia and Sr. Juan Larrea, now living in the U.S.A. From the United States came Dr. Wendell Bennett, whose status in South American archaeology is such as to make his presence at such a gathering almost essential to its success. Dr. George Kubler, who had come to Peru on behalf of U.N.E.S.C.O. to make recommendations about the repair of earthquake damage in Cuzco, was able to attend some of the sessions, and Drs. John Gillin and Ozzie Simmonds who were doing anthropological work in Peru on behalf of the Smithsonian Institute of Social Anthropology also attended. Dr. Greta Mostny came from Chile, but otherwise the Latin-American countries were but sparsely represented on the archaeological side. The absence of Mexicans, who have done so much in recent years, was particularly noticeable, but this may have been mainly due to the imminence of celebrations connected with the fourth centenary of their own university.

I cannot usefully go into the subject matter of the Congress at any length, because it was divided into sections, including history, linguistics and archives, so that the archaeological one which I attended only covered a small proportion of the business transacted. This section included ethnology, art and folklore at the beginning, and these subjects gave rise to such long and sometimes speculative discussions, that archaeology was eventually made a separate section and progress was then faster. The archaeological work included an interesting discussion, opened by Professor Bennett, on the relations between the Andean and Coastal cultures which dealt mainly with Peru itself, but Dr. Lehmann and I extended it northward into Ecuador and Colombia. As regards new work, important discoveries were announced by

two investigators. Dr. Henri Reichlen, who has been working with his wife in the northern Andes for several years on behalf of the Musée de l'Homme, gave an account of the succession he has established in the Cajamarca Region, of which a summary appeared in the *Journal de la Société des Americanistes de Paris*, 1949. He also gave a lecture, illustrated by excellent photographs, about his more recent work further east along the right bank of the Rio Marañon in the Department of Amazonas, where some very remarkable fortified towns and funeral monuments were found. The other discoveries came from the southern Andes and are due to Professor M. Chávez Ballón, of Cuzco, who has established the existence of a pre-Pucará horizon in the northern part of the Titicaca Basin, which underlies the Pucará culture studied but not yet fully published by Dr. A. V. Kidder II. This horizon can probably be correlated with the Pre-Inca Chanapata horizon of Cuzco. In the neighbourhood of Cuzco itself he believes that he has demonstrated the presence of a Tiahuanacoid horizon between Chanapata and Early Inca.

Apart from the archaeological section, there was one communication made to a plenary session which is of so much general interest that it must be noticed. The President of the Congress, Dr. Porras, has recently investigated the life of the Inca Garcilazo de la Vega in Spain, and has succeeded in filling in the period of 30 years, from c. 1560 to 1590 between his arrival in Spain and the publication of his *Comentarios Reales*, about which nothing was known. He showed how he lived with an uncle, who finally left him his property but subject to the life interest of his widow, a condition which deprived him of much of its benefit. He described how difficult it must have been for a mestizo to fit into the rigidly classified pattern of Spanish life, which doubtless roused in him a deep nostalgia for the life of his Peruvian maternal relatives. During these 30 years he occupied himself chiefly in breeding horses.

Apart from visits to places of interest in Lima, two main excursions were organized by the Congress. One Sunday there was a trip to the well-known site of Pachacamac followed by a Creole lunch at a neighbouring hacienda to the tune of two local orchestras, a very agreeable function. At the end of the Congress a number of delegates were flown up to Cuzco for three days under the leadership of Dr. Valcárcel. Visits were paid to a number of Inca ruins with which I was already acquainted, but I was extremely fortunate in getting Professor Chávez Ballón to guide me to various archaeological sites, including the pre-Inca one of Chanapata, discovered by Professor J. H. Rowe since my last visit. We also inspected the earthquake damage, which is extensive, but most of it should be capable of repair if the advice recently offered by Dr. Kubler is taken. Suggestions have been made that the Dominican convent should be moved away from the site of the Inca Temple of the Sun, since the church is seriously damaged, but this course would involve the loss of important historical associations of the Colonial Period with little gain to compensate for it. All that is necessary is a slight modification of the sanctuary of the church, when it is rebuilt, to free the famous Inca curved wall from pressure.

My visit to Peru did not end with the Congress. I had a few days to spare before leaving, and I was extremely fortunate in enjoying the generous hospitality of Señores Rafael Larco Herrera and Rafael Larco Hoyle at the Hacienda Chichín near Trujillo. Their private museum is world-famous, and Dr. Lehmann and I derived incalculable benefit from the material we saw there and from our discussions with Sr. Larco Hoyle. Equally useful to us were the visits we paid under the guidance of Sr. Larco Herrera to the most important preceramic agricultural site of La Huaca Prieta, to some Mochica irrigation works of staggering proportions higher up the Chicama Valley and to Chan-Chan, the famous Chimú metropolis.



**Lectures at the Horniman Museum, January—March, 1952**

**286** Among the illustrated lectures arranged by the Horniman Museum, Forest Hill, London, S.E.23, on Saturday afternoons at 3.30 during the first quarter of 1952, the following are of particular interest to anthropologists: 'Archaeology and Air Photography,' by Mr. W. F. Grimes

(19 January); 'Medical Belief and Practice among Primitive Peoples in the Modern World,' by Miss S. R. Burstein (26 January); 'The People of Britain,' by Professor H. J. Fleure, F.R.S. (23 February); and 'The Masked Dancers of Barotseland in Northern Rhodesia,' by Professor Max Gluckman (1 March).

## REVIEWS

## AFRICA

**The Burden of Egypt.** By John A. Wilson. Chicago (Univ. of Chicago Press) (U.K. agents: C.U.P.), 1951. Pp. xx, 332, 32 plates, endpaper maps. Price £2 5s.

**287** As the author says, 'this is not a history of ancient Egypt, but rather a book about ancient Egyptian history'; but it is none the worse for that. It attempts to assay the significance of what we now know about Egypt, from our varied sources, inscriptions, papyri, works of art and the like, and from the study of the land itself, its resources and its people. It presumes access to published collections, but quotes many interesting documents in translation, so that they may 'speak for themselves.' The dearth of such publications, even now, is lamented, all the more because a century of exploration in Egypt has accumulated so much since the earlier collections were made. Breasted's *History of Egypt* is already 40 years old.

While ancient Egyptian writers had no conception of historical time, and very little of the relations of cause and effect, they had a clear notion of the nature of the world, and of man's place in it, under all-embracing guidance from the numerous gods. This inevitably altered the presentation of political occurrences, regarded as achievements of the god-king, no less than of natural occurrences. But only rarely have we any check on these representations. Dr. Wilson claims the right to exercise 'good sense' where scientific method fails to reach impartial conclusions, and to present the ancient Egyptians in their actual surroundings, and with kings and viziers doing their best to use their 'good sense' too, under the disabilities of tradition and mythology. This he calls 'discovering the values inherent in Egyptian culture,' considering value 'in a modern sense.' It is 'perspective,' rather than specific information, that matters: but judgment must rest on information, and here he has all the qualifications of a learned and impartial guide.

This point of view explains the unusual chapter headings: The Black Land; Out of the Mud; The Search for Security and Order; The King and God; The First Illness (Dynasties 7-11); The King as the Good Shepherd; The Great Humiliation (Dynasties 13-17); Far Frontiers; Irrepressible Conflict; Where is the Glory? The Broken Reed.

There are some admirable plates and a good index.

JOHN L. MYRES

**Race Relations in Ancient Egypt, Greek, Egyptian, Hebrew, Roman.** By S. Davis. London (Methuen), 1951. Pp. xiv, 176. Price 21s.

**288** The author of this interesting book is a graduate and lecturer of the University of the Witwatersrand, Johannesburg. He has studied in Cambridge, and read the principal ancient and modern authorities. His introduction shows that he is deeply interested in the general problems of race, and the special question of anti-semitism; and he ends on a comparison between the policies of the Roman and the British Empires. But in general his book is free from tendentious matter, and sets out to display Egypt, and especially Alexandria, as a theatre of race conflicts. It is a pity that he did not carry forward his studies into the Arab and Turkish periods, where some of the old difficulties recur, and new ones arise under Moslem rule.

Within these limits Mr. Davis has managed to include much of general interest, as background to his special theme. He begins with a sketch of Greek exclusiveness under the city-state system, the

political ideals of Alexander the Great, and the wider outlook and practices of the Hellenistic Age. Then he reviews the earlier Greek contact with Egypt, and gives a careful study of the work of Herodotus. After a brief but sufficient note on Alexander's visit, and the foundation of Alexandria, he turns to the value of papyri for Egyptian history and their main contributions in the Hellenization of Egypt. Then comes a very important section on Alexandria where race relations were closest and most strained between Greeks—original colonists and later immigrants—and the native Egyptians.

A first topic, already adumbrated at Alexandria, is the Jewish element in Egypt. This was already ancient when Alexandria was founded, but Jewish refugees and traders counted for little, till urban life matured, and till the Greek kings discovered the value of Jewish fighting men. Hence the complicated problems of the status and constitution of the Jewish community in Alexandria, the antipathy between Jews and Greeks, and the effect of Greek culture on Alexandrian Judaism which reacted in turn on the Jews of Palestine, and their later literature.

The third part of the book deals with Roman relations in Egypt both with Greeks and with Jews. To explain these, Mr. Davis finds it necessary to go back to the beginnings of Roman expansion, and the remarkable and original conception of Roman citizenship which had only come to maturity a little before the Roman conquest of Egypt, and about the time of the first Roman acquaintance with Jews, both in Egypt and Palestine, and in Italy. There might have been more said about Roman administration in Egypt, but the centre of interest for Mr. Davis is the disastrous quarrel of Rome with the Jews of Jerusalem; and he has already taken account of the grievances of the Jews of Alexandria, and the wise handling of them by the Emperor Claudius, in Part II.

Mr. Davis keeps close to his authorities, often, quotes them at length, and seldom offers opinions of his own. With voluminous footnotes as well, much of the book is rather heavy reading; but it is painstaking, judicious and very free from slips—such as 'Darius Ochus' (p. 35) and Ptolemias (p. 43). On p. 41 it might be made clearer that 'Soter' is the same person as 'Ptolemy Lagos.'

JOHN L. MYRES

**La Civilisation du Tchad.** By Jean-Paul Lebeuf and Annie Masson Detourbet. Paris (Payot), 1950. Pp. 198, 50 text figs. Price 480 francs

**289** This is the first integrated account by M. Lebeuf and his wife of the results of their many years of exploration and research in the region of Lake Chad into the relics of the legendary Sao, and their cultural descendants the present-day Kotoko. But this account is not intended to be more than an interim report, and its authors look forward to many more years of intensive work in the region. Important new evidence of the extent and high quality of the Sao bronze work has in fact since been published by M. Lebeuf (*Illustrated London News*, 23 June, 1951). In this latest assemblage of bronzes is a 'libation bowl' of delicate workmanship comparable, if (as appears in the photograph) it is shaped like a 'pinched' hemisphere, to the wonderful bronze bowls described by J. O. Field (*MAN*, 1940, 1) from Igbo in Onitsha Province, Nigeria. These small bowls and some much larger ones of similar shape (perhaps derived from the shape of a calabash) might through other objects in the same series provide a link, however tenuous, with the



Yoruba-Bini bronze complex. The authors themselves see no apparent link with the art of Benin, though Raymond Lantier who writes an appendix on the bronze work of the Sao seeks to draw a parallel with the bronze-founding of Benin and Mossi.

A comprehensive and very readable account is given of the environment, legends and history of the Kotoko, together with details of the methods employed in the interrogation of informants and analysis of evidence. Fantastic tales are recorded of giants of prodigious physical strength and hunting prowess, five times the stature of normal folk; these are incidentally believed implicitly by the inhabitants of a large part of north-eastern Nigeria, many of the educated people adding that there is indisputable archaeological evidence to support the legends. Chance finds of elephant and hippopotamus bones become the remains of the extinct Sao giants. The five-foot funerary urns are their drinking cups or the playthings of their children, neck rings of bronze become bracelets, bracelets are said to be finger rings, and so on.

The archaeological finds consist almost entirely of pottery work, and the quantity and diversity of the terra-cotta figurines, ritual objects, ornaments and even 'money' lead one to suppose that clay was the chief medium for the artistic expression of the Sao. Whereas the terra-cotta tradition seems to spring from purely African origins, exotic influences are seen in the bronze work, the blue glass and the construction of town walls.

This work is well illustrated by Madame Lebeuf's drawings, but would have been enriched by the addition of a few photographic plates. There is a valuable bibliography.

BERNARD FAGG

**Two Studies in Applied Anthropology in Kenya.** By Philip Mayer. *Col. Res. Stud. No. 3.* London (H.M.S.O. for Colonial Office), 1951. Pp. 33. Price 2s. 6d.

290 The two descriptive studies in this book are of 'agricultural co-operation by neighbourhood groups among the Gusii of South Nyanza' (pp. 5-18), and 'bridewealth-limitation among the Gusii' (pp. 19-33). The first is a study of the custom by which neighbouring homesteads pool their labour resources. These co-operative groups are called *risaga* (pl. *amasaga*), and the country is divided into named and defined '*risaga* areas.' Reward for work is by means of beer parties. The effect of European civilization on this system is discussed; and, as Mr. Phillips points out in his Introduction, the *risaga* system might suggest a method for 'mobilizing the co-operative effort of peasant agriculturists for measures of rural

improvement' elsewhere. The second part of the book deals with the attempts made by the Gusii to limit the marked rise in the number of livestock payable as bridewealth which began in 1942. It seems that the Gusii expect periodical fluctuations in the amount of bridewealth, and consider that control measures are needed from time to time. The book is a useful addition to Dr. Mayer's published material on the Gusii.

G. W. B. HUNTINGFORD

**A Report on a Social Survey of Sekondi-Takoradi.** By K. A.

291 Busia. London (Crown Agents for the Colonies, for the Govt. of the Gold Coast), 1950. Pp. 164

Dr. Busia's Report, a valuable study of a rapidly growing and changing port, is admirably dispassionate in its presentation, though at times a little curt. Housing, employment, married life, schooling, municipal government and associations are dealt with in the early chapters. Dr. Busia then goes on to consider indices of social failure which include juvenile delinquency, destitution, sexual immorality and so on. The Report is concluded with some suggestions. The statistics are in the main relegated to appendices.

Perhaps the most interesting chapter is that on juvenile delinquency, with its case histories only too familiar to anyone who has ever attended juvenile courts in England. Here in Africa too the effect of the 'broken home' is sadly apparent.

In his section on married life Dr. Busia speaks of a rising divorce rate; one would be glad of some statistical evidence of this. In the section on sexual immorality he speaks of the 'breakdown of sexual morality' without indicating what were the standards before the 'breakdown.' Now, whatever were the standards of the indigenous Ahanta, among some of the northern immigrants into Takoradi pre-marital intercourse is the rule and not the exception. Again, is the woman who lives with a man as his mistress really to be classified with the prostitutes to whom the pilot boys lead their customers? Some budgets showing the monthly expenditure of the income groups enumerated on p. 20 would have been of great interest since Dr. Busia says that some of them 'do not earn enough money to cover these items (food, rent, fuel and lighting).' How then do they live?

These are some of the questions that could be raised. Another is: Do the children quoted on pp. 53f. really go to choir practice every night? They are all, however, minor points of criticism of a very comprehensive though brief Report. More extended treatment in papers of aspects of Takoradi life merely touched on in this volume will be eagerly sought by its readers.

DAVID TAIT

## AMERICA

**The Two Crosses of Todos Santos: Survivals of Mayan Religious Ritual.** By Maud Oakes. New York (Pantheon Books), 1951. Pp. xiii, 274, 22 plates. Price \$5

292 The two crosses from which the title of Miss Oakes's book is derived are to be seen in front of the Roman Catholic Church at Todos Santos, a small mountain village of the Mam tribe in Western Guatemala: a wooden one, built in remote times by the Indians, and another made of adobe and stone by the *ladinos* (Spanish-speaking *mestizos*). Together, they symbolize the twofold religious life of an Indian group subjected to the impact of Christianity for four centuries. It soon appears, though, that the properly native elements are far more important in the present-day Mam religion than the imported ones, a fact which will not surprise any student of Middle American sociology.

There is much to commend in Miss Oakes's approach and methods: she chose to live in close and friendly relations with the Indians and to show their institutions and beliefs the respect and understanding to which they are entitled. Her observations are given, as it were, as raw materials with a minimum of interpretation and discussion; and although one may feel that this is sometimes unfortunate, owing to the interest of the problems involved, it is only fair to acknowledge that she gives a very vivid and candid picture of the religious life of those Indians.

Of outstanding interest is the description of the *chimanes*, a category of priests, soothsayers and 'medicine-men,' whose

relations with the spirits and shamanistic performances constantly border on magic and religion. From the author's observations it seems possible to conclude that the priests *stricto sensu* are the *rezadores* or prayermakers, while the *chimanes* may be described as shamans. But the two groups regularly overlap, as the chief shaman or *Chimán Nam*, also called *el Rey* (the king), is normally a member of the prayermakers' college. There are nevertheless two distinct centres around which the religious life of the community revolves: one is the public cult of the *Caja real* or royal coffer, a sort of sacred box which reminds one of the *envolorios* or sacred bundles of the Aztecs, this cult being supervised by the chief prayermaker; the other is the shamanistic activity of the *chimanes*, which includes intercourse with supernatural beings, divination, healing and black sorcery. The relations between the *chimán* and his individual spirit are conceived as a personal pact, along a pattern which strongly parallels North American phenomena (the future shaman goes to the hills, waits for the revelation of his spirit, must be introduced by a confirmed shaman, etc.).

The main deities are identified with four *cerros* (mountains) near the village, and at the same time with the four Year-Bearers, the four main days of the ritual calendar of 20 days. This Year-Bearer theme is still very much alive in the Maya-speaking tribes of Guatemala (see for instance LaFarge, *The Year-Bearer People*). Most fascinating and also somewhat disappointing is the account which Miss Oakes gives of the Mam Calendar. Her Indians use the old



Maya year of 18 months of 20 days each, to which five days are added; but it appears that they do not possess the one-to-thirteen count, which means that their calendar is not a *tzolkin*. Another fact is even more anomalous: although the recognition of four Year-Bearers is obviously linked with the fact that the supplementary days at the end of each year must be named according to the never ending succession of the 20 days, the Indians of Todos Santos always end the year with the same set of four days, and the last day is always *batz*. It seems that the system has become, as it were, fossilized, perhaps owing to the fact that *batz* is the sorcerers' day and therefore must be placed at the end of the year to allow the *chimanes* to celebrate the year-ending ceremonies.

The calendar itself is closely akin to that of the Quiché tribe of Guatemala. Owing to the lack of space, I cannot go into a detailed discussion of the day names; a close study shows that they are quite similar to those of the Yucatecan Maya and of the Mexican tribes. It begins with the first year-bearer, *k'mané* or *ee* (Quiché *ee*, Yuc. *eb*, Azt. *malinalli*); the other three year-bearers are *noj* (Q. *noj*, Yuc. *caban*, Azt. *olin*), *ik* (Q. *ic*, Yuc. *ik*, Azt. *ecatl*) and *t'ce* (Q. *quiej*, Yuc. *manik*, Azt. *mazatl*).

The last day name, *batz*, means 'howling monkey' in present-day Lacandon; its Yucatecan equivalent is *chuen*, the glyph of which represents a conventionalized monkey head; the Aztec day name is *ozomatli*, monkey. Now the sacred book of the Quiché, the *Popol Vuh*, mentions two beings called *Hun Batz* and *Hun Chuen*, described as singers and orators, who are metamorphosed into monkeys. Sahagún links the *ozomatli* sign with the singers and also with black magic and the appearing of the evil spirits called *Chuateteo*. Among the Mam, *batz* is the day of the shamans (who sing and perform magic).

The four year-bearers of the Mam today are the same as those which were in use during the Maya Old Empire and among the Oaxacan tribes (see for instance the *Codex Porfirio Díaz*, where the year-bearers are *ecatl*, *mazatl*, *malinalli*, *olin*). They differ from those of the Yucatecan Maya, *kam*, *mulum*, *ix*, *canac*.

The above-mentioned points are only a few samples of the great wealth of material which Miss Oakes brings us and for which the students of Middle America should be deeply grateful.

JACQUES SOUSTELLE

#### Note

<sup>1</sup> The following table shows a comparison of the Mam, Quiché, Yucatecan and Aztec day names. The Year-Bearers are in italics.

MAM	QUICHÉ	YUC. MAYA	AZTEC
1. <i>k'mané</i> or <i>ee</i>	<i>ee</i>	<i>eb</i>	<i>malinalli</i>
2. <i>aj</i>	<i>aj</i>	<i>ben</i>	<i>acatl</i>
3. <i>ix</i>	<i>ix</i>	<i>ix</i>	<i>ocelotl</i>
4. <i>tsikin</i>	<i>tziquin</i>	<i>men</i>	<i>quauhtli</i>
5. <i>ajmak</i>	<i>ajmak</i>	<i>cib</i>	<i>cozcaquauhtli</i>
6. <i>noj</i>	<i>noj</i>	<i>caban</i>	<i>olin</i>
7. <i>tcij</i>	<i>tijax</i>	<i>eznab</i>	<i>tecpatl</i>
8. <i>tciook</i>	<i>cagüek</i>	<i>canac</i>	<i>quiauitl</i>
9. <i>hunapu</i>	<i>ajup</i>	<i>ahau</i>	<i>xochitl</i>
10. <i>imix</i>	<i>imox</i>	<i>imix</i>	<i>cipactli</i>
11. <i>ik</i>	<i>ic</i>	<i>ik</i>	<i>ecatl</i>
12. <i>akbal</i>	<i>akbal</i>	<i>akbal</i>	<i>calli</i>
13. <i>kets</i>	<i>kar</i>	<i>kam</i>	<i>cuetzpalin</i>
14. <i>kan</i>	<i>kan</i>	<i>chicchan</i>	<i>coatl</i>
15. <i>kimex</i>	<i>camé</i>	<i>cimi</i>	<i>miquiztli</i>
16. <i>t'ce</i>	<i>quiej</i>	<i>manik</i>	<i>mazatl</i>
17. <i>k'nel</i>	<i>kanil</i>	<i>lamat</i>	<i>tochtli</i>
18. <i>t'coj</i>	<i>toj</i>	<i>mulum</i>	<i>atl</i>
19. <i>t'ciik</i>	<i>tzii</i>	<i>oc</i>	<i>izcuintli</i>
20. <i>batz</i>	<i>batz</i>	<i>chuen</i>	<i>ozomatli</i>

#### The Cheyenne in Plains Indian Trade Relations, 1795-1840.

By Joseph Jablow. *Amer. Ethnol. Soc., Monograph XIX.* New York (Augustin), 1951. Pp. vii, 100. Price \$2.50.

The main articles of trade in the Great Plains were, first, manufactures (including guns), which were given in exchange for furs by the trading companies of the Northwest, and, secondly, horses which were bred in Mexico and stolen from there by the Kiowa, Comanche and other tribes. In general, therefore,

manufactures moved towards the South, and horses went from Mexico to the Northeast.

The author describes the roles of the different tribes in trading: of the Sioux, who, in closest contact with the Fur Companies, maintained an early monopoly in guns; of the Missouri village-dwellers, particularly the Arikara, who were middlemen: of the Cheyenne, who traded with these villages (and, later, with American trading posts established in their country), giving horses, which they had traded or stolen from the Kiowa and Comanche, in exchange for manufactures; and, finally, of the Kiowa and Comanche, who obtained their horses by stealing from the ranches of Mexico and the Southwest.

This is the first systematic study of trade in the Great Plains. Apart from its originality, the importance of the book lies in its conclusive refutation of two common assumptions: that Plains warfare was little more than a game; and that Plains societies had subsistence economies. In fact, as the author shows, warfare was mainly an economic activity by which men obtained horses for trade. And, the use of horses made possible a surplus which was used for trade.

The author deals briefly and clearly with a complicated subject. His book, taken with those of Mishkin and Lewis (Vols. III and VI in the same series), makes intelligible the relations between the tribes of the Great Plains in the early nineteenth century. F. G. BAILEY

#### Mandan Social and Ceremonial Organization. By Alfred W. Bowers. Chicago (Univ. of Chicago Press) (U.K. Agents:

294 C.U.P.), 1950. Pp. xvi, 407, 44 illus. Price £2 16s. 6d.

The Mandan Indians were one of the Siouan-speaking agricultural tribes of the Missouri Valley region. Their villages were important trading centres for all of the nomadic tribes of the Northern Plains and, with the coming of the white men, became commercial centres in the fur trade. Wars and epidemics greatly reduced the size of the population throughout the nineteenth century. In the United States census of 1910, the Mandan numbered 197. *Mandan Social and Ceremonial Organization* is an attempt to recapture and reconstruct the Mandan way of living; it is an ethnography in the old tradition. It consists of three parts; an introductory historical chapter and two major sections dealing with social organization and with ceremonialism.

The historical chapter is brief, too brief to present the four hundred years of history with which it deals. The result is a highly concentrated dosage of names, places and dates without the aid of maps, charts or diagrams. Part I, on social organization, covers approximately 80 pages and includes chapters on the kinship system, the life cycle and the organization of such units as the household, clan, moiety and village. Part II, devoted exclusively to ceremonialism, covers 250 pages of the text; more than half of these pages are in small print and are verbatim reports of various ceremonies. This part, unfortunately, is not in any way systematically related to Part I. There is a large appendix which contains verbatim accounts of a number of myths which presumably support the ceremonies, but this is not explicitly indicated. The book lacks balance, not only because it primarily deals with ceremonialism, but also because little attempt is made to go beyond description.

The most serious criticisms concern the author's lack of a time perspective and the doubtful validity of some of the data. It is difficult for the reader to ascertain the period of time to which the material refers. The entire text is written in the past tense and seems to refer to anything that may have existed between 1837 and 1932, the year when the bulk of the fieldwork was completed. Since the author states that no 'bundle' ceremonies 'had been performed during the thirty years preceding this study,' we must guess that most of this material refers to the nineteenth century. This procedure leads to some highly awkward and hypothetical constructions, which bear on the authenticity of the data. For instance, in 1837 'there was a feeling that in former times traditional eagle-trapping sites were divided into moieties...' (p. 33, italics mine). In addition, the reader is offered no information on the number of Mandan still living today. All we know of the population at the time the study was made is that, of 14 informants used in 1932, 11 were Mandan, and in 1947 only one of these was left.

The material which is presented was secured from these 14 old



informants, who were asked to search their minds for memories of what they had been told it had been like to be a Mandan in former days. The process of remembering is highly selective in favour of one's own interests and immediate perspectives; the Mandan themselves show this by the highly idealized versions (e.g. no tensions, no conflicts, every one in the family loved each other, etc.) they have presented to the author, versions which have been incorporated into the book. For example, we are told, 'informants claim that all tribal rites were preserved through the smallpox epidemics of 1782 and 1837.' Surely a contemporary Mandan has no better basis for claiming familiarity with the events of 1782 than anyone else. The fact that he is Mandan does not give him access to the indisputable history of his group.

This book, because of certain oversights, raises some serious problems in historical method in the field of 'aboriginal anthropology.' One is tempted to ask to what extent a reconstruction, such as the one Bowers offers us, can be authentic. Also, is it useful as social anthropology or can it serve other methodological purposes? Bowers, of course, is not concerned with these questions. If he had been he might, for example, have said something about the effect of the fur trade and commerce on the social organization of the Mandan. Or he might have attempted to explain the way in which their self and world image in relation to ceremonialism had been effected by their more violent experiences during the nineteenth century. These problems are not irrelevant to the task which the author sets for himself.

ARTHUR J. VIDICH

## ASIA

**The Hunt for the Buru.** By Ralph Izzard. London (Hodder & Stoughton), 1951. Pp. 176, 23 illus. Price 15s. 6d.

**295** When Messrs. J. P. Mills and C. R. Stonor visited the Apa Tani tribe in a previously unexplored corner of the outer Himalayas in Assam in 1945 and 1946, they received from men, whose ancestors had seen and destroyed them, accounts of the *buru*, apparently a genus of monstrous saurians which inhabited the lake that was the Apa Tani valley before the inhabitants drained it for the cultivation of wet rice. These traditions were so circumstantial that Mr. Izzard was inspired to make an expedition to investigate them. The expedition was financed by the *Daily Mail*, and this book is an account of Mr. Izzard's wild-goose chase.

The first chapter tells how the author became involved in his quest. The second is the very careful and considered report by Mills and Stonor of the Apa Tani tradition. The third is a letter from Stonor reporting Dafla accounts of living *buru* in a virtually unexplored valley some 50 miles from the valley of the Apa Tani tradition. Izzard decided to search for the living rather than the dead *buru* and the remaining chapters give an account of his expedition to the Rilo valley and his fruitless return to be the victim of an amusing journalistic gaffe. The description of the difficulties encountered, from climate, jungle, leeches, and other insect pests, in particular, is vivid; the descriptions given of Daflas, their villages, and the scenes met with in their country are graphic and realistic, and the photographs are admirable. All this, however, does not claim to be anthropology, and as for the *buru*, apparently even the Daflas themselves did not really believe in them.

The report of Mills and Stonor on the Apa Tani tradition must be considered on its own merits. Its value is not necessarily discounted by the fact that the Daflas of the Rilo valley, who speak a different language and have an entirely different culture, had a story of *buru*—the word is Apa Tani not Dafla, though used by them apparently—which proved to have no foundation. It might easily have been a transplanted myth. Mills and Stonor, then, give a very brief account of the area and its inhabitants, followed by the detailed descriptions of the *buru* given them by Apa Tani informants in two successive years together with such accounts of the *buru* and their destruction as they could gather at both visits. They describe the methods by which they obtained these traditions, and they examine the traditions soberly and critically. They were shown the exact sites at which four *buru* were said to have been killed and buried, and they are clearly inclined to believe in the survival of some quasi-prehistoric saurian up to the time when the Apa Tanis occupied their present habitat.

It is difficult for an armchair critic who has not met an Apa Tani to share this confidence with Mills and Stonor. The possibility that it is a garbled account of a crocodile is considered and rejected by them, but on what seem to me rather slender grounds. A description of the crocodile or of the gharial handed down through many generations might well have grown into something which no one of the present generation would recognize in a crocodile seen today, and the Apa Tani seem to have been isolated from the possibility of seeing crocodiles for many generations. The fact that a *buru* knocked a man into the water with its tail is strongly suggestive of the Sunderbans mugger. The description of the teeth will not fit the mugger, but that of the skull said to have been found is not

excessively remote. All this is assuming that the tradition is based on the genuine experiences of the ancestors of the Apa Tanis many generations ago; how many generations is unknown, but it must be at least seven, since the last known trace of a *buru* was the discovery of a skull by a man of seven recorded generations ago. But can any real value be attached to such a tradition? The manor of Sockburn was held by the presentation to the Bishop of Durham, on his first entering the diocese, of the falchion with which the ancestor of the Conyers family slew 'the worm, dragon, or fiery flying serpent which destroyed man, woman and child,' this falchion being handed back by the Bishop to the lord of the manor. This tenure is on record in 1396 and the service was last performed in 1826. Another estate, Pollard's Dane in Bishop Auckland, was held by the Pollard family on a similar tenure. The well from which the Lambton Worm came forth to terrorize the peasantry till slain by the ancestor of the Earls of Durham continued to exist under the name of Worm Well at any rate until the middle of the last century, and the trough which had to be filled for the worm with the milk of nine cows was shown at Lambton, as well as a bit of the worm's skin, in Surtees's time. The worm of Linton had a den in the Worm's Hole there, and was killed by John Somerville of Laristan, who received the barony of Linton for the exploit, in 1174. This worm was three Scots yards long and somewhat bigger than a man's leg and like an adder in form and colour. Somerville killed it by thrusting down its throat a turf dipped in scalding pitch at the end of his lance. His monument carved with his exploit was built into the wall of Linton church. Here are circumstantial accounts indeed, less than 1000 years old, but it is difficult to take them as seriously as the authors of Izzard's second chapter take the Apa Tani tradition.

In one point there is perhaps an error of fact; the authors say, 'The *buru* tradition has no parallel in the Assam-Burma frontier. Small lakes and pools are scattered about the region . . . but there is no case known where any legend at all comparable with that of the *buru* exists . . . Lacustrine monsters are no more associated with the general run of legendary beliefs among mountain peoples than are sea serpents' (p. 40). But there is a monster of some sort in a reputedly bottomless pool near Viswema village in the Angami Naga country which people are afraid to bathe in; the Pala lake in the Lakher country (see Parry, *The Lakher*, p. 561) issued from a hole in which a monstrous snake lived that devoured children and was cut into pieces by the villagers; earthquakes, for the Thado Kuki, are caused by a great serpent with seven nostrils that lives underwater and, chasing its tail round the earth, occasionally bites it. It devours men by catching them underwater and using them as pillows. Lushei, Shan and Abor all have a similar account of earthquakes. In the case of the Thado at any rate the fear of this *gulpi*, which may be encountered in any deep water, is very real. The Apa Tani's account of the *buru* is very matter of fact, but the story of the magic discs which killed the *buru* of their own motion is not such as to encourage a belief that the account of the *buru* is really factual. Accounts of real and striking events can no doubt be handed on for several hundred years, but in so far as they are oral and not written they tend to collect a large number of non-veridical accretions. One must readily agree with the authors of this chapter that the *buru* tradition cannot be regarded as historical fact 'unless confirmatory evidence of a more concrete nature can be produced.'

J. H. HUTTON



**Sumangat: L'Ame et son Culte en Indochine et en Indonésie.**

By Jeanne Cuisinier. Paris (Gallimard), 1951. Pp. 267, 16 plates. Price 650 frs.

296

The title of this book comes from a word that embraces at once the idea of vital force and of personal spirit, notions described in modern jargon as animatism and animism. In Malaya, for centuries a Muslim land, the word *sẽmangat* (a spelling more orthodox than *sumangat*) has been superseded by an Arabic loan word *kẽramat* to distinguish ancestral spirits, but among those Sumatran hill folk the Bataks *tondi* stands for vital force, which after a person's body has decayed becomes his ghost (*begu*=the Malay *hantu*), though only the ghosts of those with wealth and a numerous progeny become *sẽmangat*, a noun derived by philologists from *sangar*, a word denoting 'excess'. This posthumous hierarchy, as Dr. Cuisinier terms it, can be traced clearly even in Muslim Malaya in adjurations to dead chiefs and shamans and in the belief of its aboriginal races in a heaven reserved for those receptacles of excessive vital force. Dr. Cuisinier's book was published before Martin Råsänen had written his startling article on 'Regenbogen—Himmelsbrücke' (*Studia Orientalia*, Vol. XIV, Helsinki, 1950). But she has noted how a hierarchy of human spirits is found also among the Chinese and Polynesians.

Her net has been cast perhaps too wide for the rigorous method of modern anthropology, beliefs about the spirit or soul being

collected from Senegal, West Africa, British Columbia, Oregon, Transylvania and Bohemia, Christian saints like Tertullian and Augustine, Plato and later philosophers as well as the Vedas, Upanishads, Buddhism and Islam. But the discursive chapters that make up the first half of the book are extremely readable, an admirable quality scholars are apt to despise, and Dr. Cuisinier is well aware of the inadequacy of the term 'spirit' to represent an idea, 'with which are associated breath, sometimes shadow, shape and invisibility at the same time, often corporeality and always force' (p. 29).

Scientists will prefer the second half of the book, dealing as it does with her own researches among the Mĩong of Indochina, Thais and Malays. On the Mĩong she has already to her credit an exhaustive and valuable work, and years ago she visited Malaya and wrote on 'Dances magiques de Kelantan'; for the Malay archipelago she has studied the work of Dutch ethnographers.

On p. 214 *Chenero Wasi* is a dialect form of *Chẽndẽrawasih*, a Malay loan word from the Sanskrit for the 'bird of paradise.' *Jinjang* (p. 227, l. 8) is the 'person visited by a spirit.' On p. 242 *Sidi guru*='efficacious be my teacher,' and there should be no comma after *bẽrkat*, the line meaning 'By virtue of my use of this spell.'

There is a useful bibliography and a good index.

R. O. WINSTEDT

## EUROPE

**A Natural History of Man in Britain.** By H. J. Fleure. London

(Collins), 1951. Pp. xviii, 350, 32 colour plates, 32 plates in black and white, 76 line drawings. Price 21s.

297

In these days of shoddy publishing and production of indifferent material it is a joy to receive value for one's money when buying books. This book is good value. It is modestly written by one who is master of his subject, who understands that simple English is the best medium of expression, and has no affectations. Professor Fleure has already proved himself possessed of a remarkable ability to condense complicated matters into intelligible and readable form be they produced by historian, anthropologist, geographer or naturalist. But he is not content with drawing from the writings of others to make his own deductions, for he is a fieldworker too and a shrewd observer of what goes on around him whether politics or the possibilities of the latest scientific discoveries. The glints of humour appearing here and there among the pages help to give the reader a book which makes such easy reading.

The object of the book is best explained from the editors' preface:

'In the synthesis of British ecology that the *New Naturalist* series is trying to make, it seemed to us that an essential and vital element was man himself in his relationship, through the ages, with the very varied natural environment afforded by the British Isles. We believe that the study of the natural history of man in Britain can contribute to the view that he must be regarded as one with nature. We believe that the search for a true relationship of man and his environment can alone save our beautiful islands from wrongful changes, from the degradation and destruction which may result equally from unplanned development as from wrongly conceived physical planning.'

Professor Fleure gives his aims in practical fashion:

'The aim of this book, a picturing of British life, is to stimulate interest in our evolution and in its portrayal in our Museums. The museums of local culture as well as the Folk Culture department of the National Museum of Wales at Cardiff and at St. Fagan's Castle point to the need for a great effort to collect and preserve vanishing types of objects, especially in rural life. The strengthening of our museums of local culture, the collecting and storing of material in danger of being lost, and perhaps the making of an English Museum and a Museum of Scottish Life are aims to be cherished.'

No one has done more than Professor Fleure to bring about the creation of the much needed English Museum.

Of the sixteen chapters comprising the book, the first seven (roughly half the book) deal with the periods covering the Stone Age to the Sea Rovers; the remaining chapters cover the modern times (1066-1949). The ordinary reader will find chapters dealing with the development of dwellings, villages and hamlets, towns, church and castle, all model essays, giving, to use the author's own words, 'evidence of outward and visible signs of the mutual adaptation going on between the peoples of Britain and their environments.' There is a pleasing chapter on the evolution of clothing. Readers of MAN will find particularly interesting the chapter (chap. 9) dealing with the physical characteristics of the people. This embodies results of researches carried out by the author and others since 1905, aided latterly by a grant from the Leverhulme Trust.

Illustrations are excellent, though the black and white plates are perhaps preferable to the coloured ones. Praise is due to Miss Alison Birch for her delightful text figures which harmonize so well with the set-up of the book and the author's easy style.

In conclusion it might be safe to prophesy that long after the South Bank Exhibition has disappeared from the memory of most people, this book will be read as a true picture of British life.

THOMAS W. BAGSHAWE

**Welsh Furniture: An Introduction.** By L. Twiston-Davies and H. J. Lloyd-Johnes. Cardiff (Univ. of Wales Press), 1950.

298

Pp. viii, 54, 106 illus. Price 21s.

This is a book of convenient size produced on similar lines to the catalogues of English furniture and woodwork published by the Victoria and Albert Museum. But the text has the appearance of having been spun out by the printer in an attempt to balance with the 126 excellent plates. It owes much to the established works *The Age of Oak*, by Percy Macquoid, and *The Dictionary of English Furniture*, by Percy Macquoid and Ralph Edwards. A series of extracts from inventories of the sixteenth to the end of the eighteenth centuries forms an interesting appendix. One would like to have seen more of these.

Turning to the illustrations, No. 8, the carved tester of oak from Derwydd, Llanybyddie, Carmarthenshire, dated as c. 1500-30, seems too romantic to be true and does not look the type of thing to have been associated with anybody's bed at such an early period. It is observed that the bed was renovated and restored in the nineteenth century. Nos. 17 and 18, modern furniture, fit in most unhappily with the general run of illustrations and might well have been omitted. No. 19 is a poor photograph of an oak buffet described as



'perhaps the most outstanding piece of Welsh domestic furniture in existence.' The text says the buffet was made for the celebrated Sir John Wynn of Gwydir (1553-1626), but the description beneath the plate gives the date as c. 1535. Whilst the authors mention the fact that it is fully described by Percy Macquoid in his *Age of Oak*, they do not point out that since the time Macquoid produced his coloured illustration of it, it has mysteriously assumed a carved base moulding and grown two curious front feet! It would be interesting to know the present whereabouts of this important piece of furniture.

However, those interested in the study of early furniture should add *Welsh Furniture* to their bookshelves. THOMAS W. BAGSHAW

### Änderungen der Körperform beim Erwachsenen Menschen.

By Ernst C. Büchi. *Anthropologische Forschungen, Part I. Vienna, 1950. Pp. 44*

**299** In this growth study on adults, a sample of 129 males and 67 females in a Swiss village were measured twice, over a nine-year interval, in terms of the standard body and head dimensions, and the results of the two investigations were compared. Synthetic growth curves were then constructed for the ages 20-75. An absolute average increase in stature of 0.66 centimetre occurred

between the ages of 20 and 29 in males (a somewhat similar increase occurred in females also), and a further very slight increase (0.09 centimetre) between 29 and 37. Increases in absolute sitting height were greater and continued for a longer time. Büchi believes that this latter growth is due to changes in the intervertebral disks, and that this is reflected to a less extent in total stature (less because it is counteracted by a decrease in leg length). Other authors have suggested that a more probable explanation of increased sitting height is simply the deposition of fat in the gluteal region; and in any case it is quite possible that the stature increase took place in the early part of the 20-29-year period as the ordinary terminal growth phase. It would not be warranted to conclude from this study that stature increases continuously until the mid thirties; however, the necessity for making observations of adult stature on fully grown individuals is granted, and has already been pointed out by Morant. Although Büchi's method has some interest, it is very inadequate in terms of evaluating the measurements used, particularly those of the face, and of physiological explanations of age changes. On the whole, it seems very little of an improvement over the traditional cross-sectional growth study, the time interval being so large (nine years) as to obscure the significance of changes.

DAVIDA M. WOLFFSON

## CORRESPONDENCE

**The Founders of the Zimbabwe Civilization.** Cf. MAN, 1949, 80, and 1951, 280

**300** SIR,—In criticizing my article on 'The Founders of the Zimbabwe Civilization' Mr. Schofield finally makes the suggestion that it may be useful to compare the royal institutions of the Monomotapa state with those of the Bahima-Batutsi. These latter peoples are generally supposed to be Hamites who entered Unyoro and Uganda from the north-east, and therefore from that very south-western Gallaland whence it is clear that Mas'udi's Waqlimi came. This suggestion is in direct opposition to his earlier expressed difficulty in believing that Mas'udi's early tenth-century statements about the Waqlimi's empire refer to the country that was later known as the Monomotapa's empire, that is to say Southern Rhodesia.

Mr. Schofield finds three reasons for doubting that the Sofalah of Gold about which Mas'udi tells could be the well-known Sofalah near Beira. First, that Marconnes' study shows a difference of opinion as to the identity of Mas'udi's Sofalah. Secondly, that Sofalah near Beira was not occupied by the Arabs until A.D. 1150, while Mas'udi was in East Africa in A.D. 916-917. Thirdly, that the name could only apply to the coastal belt and that it is therefore most improbable that Mas'udi had any information about the interior. Hence, Schofield concludes that when telling of the Waqlimi Mas'udi was certainly not referring to the country that is now Southern Rhodesia.

Schofield's first doubt is based on Marconnes' evidence that there may have been two Sofalahs. But even if there really were two, Marconnes shows that the Sofalah of Gold would have been far away to the south of the other one, which he distinguishes as the Sofalah of Dendema and nicknames 'the Sofalah of Iron.' This Sofalah of Iron Marconnes puts at Cape Delgado, and one has to go a long way to the south of this before reaching Beira and the medieval Sofalah whence came gold. Surely, then, instead of casting doubt on the identity of Mas'udi's Sofalah of Gold with the medieval and modern Sofalah, Marconnes' study emphasizes the identity of the two.

As to the second doubt, one may ask whether the occupation in A.D. 1150 is not evidence that the country had been known before that date? In fact the occupation then suggests that by that time the country had become sufficiently well-known to make it worthwhile to found a permanent settlement there. But beyond this, surely the question is clinched by Mas'udi's statements that gold was exported thence. Why should we doubt that his Sofalah was the famous one near Beira whence in due time the Portuguese got gold?

The Portuguese evidence supplies the answer to Schofield's third difficulty, his belief that Mas'udi could not have had any information as to the interior of the continent. The name Sofalah naturally applies to the coastland as Mr. Schofield insists, but that is no reason why Mas'udi should not have heard reports of the high tableland in the interior. In the fourteenth century A.D. Ibn Batutah knows that the gold dust was not found on the coast, but was brought to Sofalah from elsewhere. He says 'between Sofalah and Youfi (Noufi), in the country of the Limiyin, there is a month's journey, and from Youfi the gold dust is brought to Sofalah.' Thus the gold came down to the coast whence it was exported, and with it stories of its country of origin naturally came too. Later it was on the coast and at the Sofalah near Beira that the Portuguese not only obtained their gold, but also heard reports of an empire in the interior whence it came, just as Ibn Batutah did some two hundred years before them and as Mas'udi clearly did four hundred years and more before that. In Portuguese days the empire was called that of the Monomotapa, and de Goes, for instance, definitely says that it lay 'in the interior (*no sertão*) of this land of Sofala.'

Of course, as I noted in my article, the place which the Waqlimi 'chose as the seat of government' in the eighth or ninth century need not of necessity have been the site which we now know as Great Zimbabwe. There are many other ruins scattered far and wide all over the country. It was for these reasons that I used the generic 'Founders of the Zimbabwe Civilization' as the title to my article and not the specific 'Founders of Zimbabwe.'

The fact that there now seem to be archaeological difficulties in accepting the foundation of Great Zimbabwe in the ninth century or so does not do away with Mas'udi's information. That information clearly is that at the beginning of the tenth century there was, and had been for some time, an aristocracy of Galla origin in the hinterland of the Beira Sofalah, that is to say in Southern Rhodesia. Oxford

G. A. WAINWRIGHT

### Notes

<sup>1</sup> In opposition to the general view Crabtree suggests (*J. Roy. Anthropol. Inst.*, Vol. LIII (1923), pp. 484-88) that the Bahima at any rate might have come from the north-west and would have been related to such people as the Fula of West Africa.

<sup>2</sup> C. Deffremery and B. R. Sanguinetti, *Voyages d'Ibn Batoutah*, Vol. II, p. 193.

<sup>3</sup> G. McC. Threl, *People of South-Eastern Africa*, Vol. III, p. 54 = p. 128.





# MAN

*A Monthly Record of Anthropological Science*

**GOLDEN JUBILEE 1901-1951**

**The Gold Mask of King Kofi of Ashanti**  
(Plate A, in colour)

**The Origin of MAN**  
*Sir John Myres*

**The Origin of Pictorial Art**  
*G. D. Hornblower*

**Primitive Transport Vehicles in Ulster**  
(with 4 text figures)  
*J. M. Mooney and G. B. Thompson*

**Proceedings of the Royal Anthropological Institute**

**Shorter Note**

**Reviews**

**Correspondence**

**U.N.E.S.C.O. on Race ; Social Anthropology, Past and Present ; The Dentition of the Australopithecines**



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# MAN

*A Monthly Record of Anthropological Science*



Notes on Some Neolithic Implements from Borneo

(with Plate B, and a text figure)

*Tom Harrisson*

Some Dimensions of the Milk Teeth of Man and the Living Great Apes

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February 20 *Theories of Structure of Social Groups.* R. F. Fortune, Ph.D.

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- March 6 At 5.30 p.m. at the Institute. *The Date of the European Megalithic Tomb (illustrated)*. Glyn E. Daniel, M.A., Ph.D.  
March 17 At 10.15 a.m. University College, Gower Street, W.C.1, Eugenics Theatre. Special All-Day Meeting. *Blood Groups and Anthropology*. Full programme to be announced.  
March 20 At 5.30 p.m. University College, Gower Street, W.C.1, Physiology Theatre. *Central Australian Sacred Totemic Ceremonies (colour films)*. P. G. Strehlow.

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*A Monthly Record of Anthropological Science*



**Corbelled Stone Huts in Southern Africa**

(with Plate D and 3 text figures)

*James Walton*

**Stone Age Man's Use of Power**

*B. A. L. Cranstone*

**Obituary**

*Alexander Low, 1868-1950*

**Reviews**

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## MEETINGS OF THE INSTITUTE IN APRIL

*at 5.30 p.m. at the Institute*

April 3	<i>Some Native Authorities in Nyasaland. Miss Lucy Mair, M.A., Ph.D.</i>
April 17	<i>Anthropology and Education: Proposals for Collaboration. Professor Margaret Read, C.B.E., M.A., Ph.D.</i>

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(with Plate E and 3 text figures)

*Philip Dark*

A Yao Girls' Initiation

*Dr. Lucy Mair*

Proceedings of the Royal Anthropological Institute

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### MEETINGS OF THE INSTITUTE IN MAY AND JUNE

at 5.30 p.m. at the Institute

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 June 26 *Annual Meeting. Presidential Address. Some Features of Nuer Religion.* PROFESSOR E. E. Evans-Pritchard, M.A., Ph.D.

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**Tribal Sculpture and the Festival of Britain**

(with Plate F and a text figure)

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**A Flint Blade Workshop near Gaziantep, South Turkey**

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The present shade of pale yellow paper, used for the cover of MAN for the past two-and-a-half years, has suddenly become unobtainable, and stocks suffice only for the present issue. The July and subsequent issues for this year will be covered in a deeper shade of yellow, which may well seem less pleasing to the eye for many readers. Consideration is being given to other possible papers for the 1952 volume; the Hon. Editor hopes to preserve the traditional association of the colour green with MAN.

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Two Eritrean Rock Sculptures Probably of Coptic Origin  
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*Sannie Drew*

Native Copper in Relation to Prehistory

*H. H. Coghlan, F.S.A.*

Proceedings of the Royal Anthropological Institute  
Blood Groups and Anthropology: A Symposium

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*Professor Vinigi L. Grottanelli*

A Pot from Nag Hanish near Mersa Matruh  
(with a text figure)  
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*A Monthly Record of Anthropological Science*



**The Rock Engravings of Kupgallu Hill, Bellary, Madras**

(with Plate I and a text figure)

*Colonel D. H. Gordon, D.S.O., O.B.E.*

**A Note on Taste-Blindness in Kenya Africans and Arabs**

*Dr. A. C. Allison*

**Proceedings of the Royal Anthropological Institute**

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(with Plate J)

*G. A. Wainwright*

The Cultural Process in India

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Obituary

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November 13 *Kalesa* (evil eye). Miss S. Zenkovsky.  
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*Dr. J. S. Weiner*

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